

SCIENCE.

FRIDAY, OCTOBER 17, 1884.

COMMENT AND CRITICISM.

WE publish this week a chart of the circum-polar regions, showing the thirteen northern stations selected by the international commission for simultaneous observation in magnetic and meteorological phenomena, together with a brief statement of the work done at each. That fifteen arctic expeditions, comprising not less than two hundred men in all, should be sent out and return without loss of life attributable to the peculiar climate or conditions of the arctic regions, except in the one case where succor had not been provided as directed and expected, offers a suggestive lesson to those who, without examining the subject, are inveighing against the dangers of arctic research.

As the outcome of sexual selection, blue eyes are to disappear, at least from Europe. So predicts Mr. Alphonse de Candolle, in his paper on heredity in the color of the eyes in the human species, recently published in the *Archives des sciences*. In investigating the subject of heredity, it occurred to De Candolle that the color of the iris offered the best outward and visible sign. It is conspicuous; it cannot be masked by artifice; after early childhood it does not vary with age, as does the color of the hair; and the character is, on the whole, distinct. For, according to him, there are only two sorts, — black, or rather brown eyes, and blue; gray eyes being reckoned as mere varieties of the blue. From the working-up of the statistics, in part from series of observations made for the purpose, it appears, that, when both parents have eyes of the same color, 88.4 % of the children follow their parents in this feature; and, of the 11.6 % of children born with eyes of other than the parental color, a part must be attributed to atavism, that is, to intermittent heredity.

But the curious fact comes out, that more females than males have black or brown eyes, in the proportion, say, of 49 to 45 or of 41 to 39. Next it appears, that, with different-colored eyes in the two parents, 53.9 % of the progeny followed the fathers in being dark-eyed, and 55.9 % followed their mothers in being dark-eyed. An increase of 5 % of dark-eyed in each generation of discoloured unions must tell heavily in the course of time. It would seem, that, unless specially bred by concolorous marriages, blue-eyed belles will be scarce in the millennium.

ARROPUS of the Bernhard Maimon collection of oriental antiquities, and of the Wolfe expedition to Chaldaea, it is instructive to note the growth of interest in Semitic study in America. The Semitic summer schools, under the inspiration of Dr. Harper and his co-laborers, attract from a hundred to a hundred and fifty students each year, chiefly, of course, for the study of Hebrew. The fact that Hebrew has been studied almost exclusively by candidates for the ministry has caused the language to be regarded as having only theological significance, and has obscured its scientific value. In some American institutions a change has taken place. At Harvard and at Johns Hopkins universities the chief interest in Semitic studies is intended to be a scientific interest. It is still true that most students who elect Hebrew expect to become ministers, but this is not the case with the kindred languages.

THE remarks made in the Electrical conference at Philadelphia by Mr. Preece, the superintendent of the British postal telegraph and telephone system, upon storage-batteries, were especially interesting, both from his account of his successful use of the original Planté form in lighting his own residence, and from the refreshing frankness of his introductory remarks, in which he stated that there had

been more lying about storage-batteries in general, during the last few years, than about any other commercial scheme before the public. Thus far, these batteries do not appear very prominently in this country. In view of the novelty and importance of the subject, both scientifically and commercially, it is to be hoped that the competing systems may be submitted to thorough tests by the boards of examiners of electrical exhibitions.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Minerals near Philadelphia.

PERMIT me to call Philadelphia mineralogists' attention to a new locality for garnets and green muscovite. The garnets are found in a small quarry of talcose rock, about one mile below Lafayette station, on the Pennsylvania and Susquehanna valley railroad. The quarry is a short distance below the soapstone quarry, and on the edge of a small stream. The garnets are very fine in color and shape. Green muscovite occurs plentifully a few hundred feet below the garnets in the side of the railway-cut.

JOSEPH T. MEEHAN.

Philadelphia, Oct. 6.

The Delaware estuary.

In your notice (No. 86) of the 'Estuary of the Delaware,' you erred in the authority for the surveys. The hydrography upon which the study was based was executed by H. L. Marindin, Lieut. H. B. Mansfield, and Lieut. E. B. Thomas, assistants in the coast and geodetic survey.

J. A. SULLIVAN.

Boston, Sept. 27.

[We thank our correspondent for calling our attention to what was an accidental omission in our notice of the recent report of the coast-survey study of the 'Estuary of the Delaware.' — ED.]

American pearls.

I beg leave to ask the assistance of the readers of *Science* in gaining information regarding the finding of American pearls in either fresh or salt water; also the weight, color, lustre, and value of the same, with the name of the mollusk in which they were found, and date of finding.

A preliminary paper on this subject was read at the Philadelphia meeting of the American association. The paper will be published in full by the U. S. fish-commission. Due credit will be given for any information.

GEORGE F. KUNZ.

With Tiffany & Co., New York, Oct. 6.

A wider use for scientific libraries.

I noticed in the last number of *Science* a proposition to render the libraries of the various scientific societies more useful by circulating the books somewhat by mail, among persons located in small towns.

If those having charge of those libraries knew what

a blessed boon such an arrangement would be to a man situated as I have been for a few years, I am sure they would heartily second the proposition. Colleges are often located in small towns, and are very poorly supplied with the means for scientific study or investigation. Professors in such institutions would be delighted with any arrangement, not involving *very great* expense, which would give them access in any way during term-time to a good scientific library. Would not some such arrangement as this be a wise one? — Require a person wishing for the privilege of taking books from the library to give bond for a sum sufficient to meet all possible liabilities, and charge to his account all the actual expenses incident to packing and mailing or expressing books to him, and also any books not returned. Charge him, also, a small annual fee for the use of the books. In that case, he would pay only the actual expenses, and for the use of the books.

I earnestly hope our scientific societies may consider this question, and give to those of us who are isolated from the rest of the world, in small colleges and small towns, the benefit of the wealth of learning idly hoarded up in their libraries.

W. Z. BENNETT.

Wooster, Wayne county, O.,
Oct. 7.

Systematic earthquake observation.

The mention of my name in several recent articles in your columns and elsewhere may excuse the seeming egotism of the proposal which is the object of this letter.

I am much interested in the recent suggestions of *Science* looking toward the closer intercourse of those who are interested in practical seismology. We have not in the United States, at least in the eastern part, any such promising field for observational work as that occupied by the Seismological society of Japan; and the number of persons at present interested in the study is not large, perhaps too small to make advisable the formal organization of a seismological society. But my records, kept now for a dozen years, make it quite evident that earthquakes, even on the Atlantic seaboard, are by no means such infrequent phenomena as is generally supposed; and I am convinced that systematic instrumental observation would largely increase the number by the detection of minor shocks and tremors which now pass entirely unrecognized and unsuspected.

As to the second point, also, it is quite probable there may be more persons interested in the subject, and willing to do some work for it, than are known to me. In the effort to find out the number and the names of such persons, I am quite willing to serve as the medium of communication for the present; and I would therefore venture to suggest that all such persons communicate with me, either through your columns or by mail directly, with the view of ascertaining whether we are sufficiently numerous to make concerted observational work possible and desirable.

C. G. ROCKWOOD, jun.

Princeton, N.J., Oct. 10.

The prime meridian.

Permit me to add to your remarks in No. 88 of *Science*, concerning the present confusion resulting from too many initial meridians, a few facts from a recent German periodical. The 'nautical almanacs' published by England, Germany, France, and the United States, refer, in part at least, to the meridians

of Greenwich, Berlin, Paris, and Washington, respectively.

In German geographical maps the meridian of Ferro is used, for the most part, while this meridian does not pass through the *island* of Ferro at all. The so-called meridian of Ferro is assumed to be exactly 20° west from Paris, while the island is only 17° 50' west from Paris.

The new topographical maps of the Prussian land-survey are based on the assumption that the Berlin observatory is 31° 3' 41.25" east from Ferro meridian, while more recent telegraphic determinations place the Berlin meridian 11° 3' 27.9" from Paris.

It is to be hoped that the result of the conference will not be a new international meridian, inconvenient at first for all nations alike.

HORACE ANDREWS.

Albany, N.Y., Oct. 11.

PSYCHICAL RESEARCH IN AMERICA.

THE adjective 'psychical' has come, through the use made of it by the English Society for psychical research, to be the label for a special class or group of phenomena, which to the unthinking are outright marvellous, even awesome, and to the thoughtful, either interesting or incredible, according to the individual mental cast. A few English scientific men believed that behind all the jugglery and deception of spiritualism there lurked a foundation of reality, perhaps grossly misinterpreted, but still of reality. That belief led to the formation of the active society named above, the work of which has already been noticed in *Science* (iv. 40).

The evidence published by this society goes to show that there are a number of more or less rare psychological effects which are most singular, and so unlike what the orthodox psychology of the day admits, that no explanation of them can yet be offered. The effects are mysterious not only as to their cause, but also as to their nature. One of them, hypnotism, was still scoffed at by the sensible until within a few years, but is now by common consent admitted even into the society of the best phenomena. Another of them, thought-transference, is still begging for a general acknowledgment of its good standing, for there are those who avow their own wisdom through the announcement of an unreasoned disbelief in the transmission of thought from one person to another by any except the ordinary channels: if the transmission appear to occur, it

is to be explained by some trickery, — so say these persons, and they have done with the matter. Now, among others of less prejudiced opinion are a number of American scientific men of acknowledged ability and unquestioned integrity, who maintain that the evidence in regard to this and other psychical phenomena cannot be thus set aside by a vague general accusation, but calls for further and more rigid investigation.

Prompted by the enthusiasm and suggestions of Prof. W. F. Barrett, one of the most active members of the English society, and supported by their conviction of the serious nature and value of psychical inquiries, the gentlemen alluded to above have decided to form an American psychical society to promote systematic study of the obscure and abnormal facts alleged to exist by trustworthy observers.

They join in this enterprise cautiously, having previously satisfied themselves that the testimony is so good that it must be received as raising a series of problems, to settle which would be interesting and important. The occurrence of thought-transference is naturally met at first by sober minds with incredulity; but, now the evidence on the subject is published, mere incredulity no longer suffices: either to prove or to disprove the reality of the transference would be equally desirable. If it be an error, it should be unmasked: if it be a reality, the discovery must appear to us momentous. In any case, there is a plain and interesting scientific duty to be performed.

Psychical research is distasteful to some persons; for it touches upon spiritualism, and to them seems akin to it. Now, spiritualism is an evil in the world, — in America it is a subtle and stupendous evil; a secret and unacknowledged poison in many minds, a confessed disease in others, — a disease which is sometimes more repulsive to the untainted than leprosy. Spiritualism has two supports, — the first trickery and deceit, the second the obscurity and inexplicableness of certain psychological processes and states. It is rational to hope that the first support without the second would soon lose its influence. The strength

of spiritualism is protected by the utter mystery which screens certain mental and nervous conditions from the light of explanation. As of others, so the basis also of this superstition is, in one word, ignorance.

To those gifted with a clearer intelligence and purer moral sense, there is a moral duty in one aspect of the proposed studies. A hope that psychical research may liberate us from a baneful superstition is a stimulus to inaugurate the work of the American society; yet a scientific man cannot calculate all the after-effects of his labor, but must toil for the truth with blind devotion. It will be the endeavor of the new society to ascertain the truth in regard to the alleged psychical phenomena, by means of experiments of unquestionable accuracy, conducted with unprejudiced independence: it will try to steer safely between the Scylla of scoffing and the Charybdis of charlatan spiritualism.

The names of the present leaders of the movement in America are a sufficient guaranty that the investigations will be thorough and serious: we shall await their outcome with great interest, and we hope, meanwhile, that the society will receive liberal public support and encouragement.

THE INTERNATIONAL POLAR STATIONS.

Now that the result of the arctic sojourn of the various parties is determined, so far as concerns the safety of their *personnel*, and the manner in which they were able to carry out the programme of the international commission, it may be interesting for the readers of *Science* to briefly review the whole topic. Including Finland, ten countries participated in the work; namely, Germany, the United States, Denmark, Austria, Sweden, Norway, Russia, the Netherlands, Russian Finland, and France. Fifteen primary stations were contemplated, of which two in the southern, and twelve in the northern, hemisphere were successfully established, all of which, it is believed, carried out the observations prescribed by the commission. The several stations were as follows:—

1. Discovery Harbor, Lady Franklin Bay, established by the United States. The party

consisted of Lieut. A. W. Greely, U.S.A., assisted by Lieuts. Kislisbury and Lockwood; astronomer Edward Israel; Octave Pavy, M.D., surgeon; two Eskimo hunters; four signal-corps observers, and fourteen petty officers and enlisted men. This expedition left St. John's, Newfoundland, July 7, 1881; arrived at Disco, July 17, and at their station, which was named Fort Conger, Aug. 12. The position of the station is approximately latitude $81^{\circ} 20'$, longitude $64^{\circ} 58'$ west of Greenwich. The *Proteus*, after landing the party and stores, sailed on her return about Aug. 26. Efforts were made to reach this station in 1882 by a party on the steamer *Neptune*, and in 1883 by one on the *Proteus*, but both failed in the attempt; nor was a suitably large supply of provisions landed for the support of a retreating party when opportunity offered. Aug. 9, 1883, the observations having been carried on successfully, the party in good condition retreated to the vicinity of Cape Sabine, finding an insufficient supply of provisions and no rescuing party. The melancholy result need not be recapitulated. Lieut. Greely and six men, one of whom afterward died, were rescued June 22, 1884, by the relief expedition under Capt. W. S. Schley, U.S.N., in the ships *Thetis* and *Bear*. The remainder perished of want and exposure, except one man shot for theft and mutiny, and one Eskimo accidentally drowned. The exact state of the records of this expedition has not been made public; but it is believed that the international programme was carried out, while a large amount of valuable geographical knowledge was attained.

2. Kingava Fiord, Cumberland Inlet, in latitude $66^{\circ} 36'$, longitude $67^{\circ} 13'$ west of Greenwich, established by the German government. This expedition, commanded by Dr. W. Giese, sailed from Hamburg, June 27, 1882, and arrived at its destination, Aug. 12; the vessel returning Sept. 8, the regular work of the station having begun the previous day, and all the observations in good running order by Sept. 15. The expedition returned to Germany in August, 1883, having carried out the international programme, and obtained valuable ethnological information in regard to the Eskimos, without mishap or serious illness of any of the party.

3. Nain, Labrador, in latitude $56^{\circ} 30'$, longitude $62^{\circ} 0'$ west of Greenwich, established under direction of Dr. R. Koch by the German government. The doctor left Hamburg, July 7, 1882, arriving in Labrador, Aug. 10. Five auxiliary stations were established by the co-operation of the Moravian missionaries, and

the full programme carried out at the Nain observatory. This station is too far south to be indicated on our chart.

4. Godthaab, Greenland, in latitude $64^{\circ} 12'$, and longitude $51^{\circ} 42'$ west of Greenwich, established by the Danish government under direction of Adjunct Paulsen of the Meteorological institute. The party, consisting of six persons, left Copenhagen, May 17, 1882, arriving at Godthaab, Aug. 1. It carried out its work with success according to the programme.

5. Fort Rae, on the northern arm of Great Slave Lake, in latitude $62^{\circ} 38'$, longitude $115^{\circ} 25'$ west of Greenwich, was established by the co-operation of Great Britain and Canada, under the auspices of the London meteorological office. The party commanded by Capt. Dawson left London, May 11, 1882, and arrived at its destination in August. It carried the international work to a successful conclusion, and returned to civilization safely in September, 1883.

6. Point Barrow, Alaska. The station at Ugluamie, a short distance from the Point, in latitude $71^{\circ} 18'$, and longitude $156^{\circ} 40'$ west of Greenwich, was established by the United States under the auspices of the Army signal-office. It was commanded by Lieut. P. H. Ray, U.S.A., and left San Francisco, July 18, 1881, arriving at its destination, Sept. 8. Observations began Oct. 17, but the full series did not commence until Dec. 1. They were carried on with unimportant interruptions until Aug. 27, 1883, when the party returned safely to San Francisco, having carried out the programme, and obtained valuable observations on the natural history and geography of the region.

7. Jan Mayen Island, at Marie Muss Bay, in latitude $71^{\circ} 0'$, longitude $8^{\circ} 36'$ west of Greenwich. This expedition was sent out from Austria at the expense and under the supervision of Count Wilczek, and was commanded by Lieut. Wohlgemuth of the Austrian navy. It left Tromsø, June 20, 1882, and was safely landed on the island of Jan Mayen by July 13. Observations were begun Aug. 1, and carried out with fidelity and success. They were reached by Aug. 3, 1883, and arrived at Vienna on the 22d, having enjoyed perfect health during their absence, and amassed rich collections of photographs, and of the fauna and flora, etc.

8. Cape Thorsen, Spitzbergen, in latitude $78^{\circ} 30'$, longitude $15^{\circ} 30'$ east of Greenwich, was selected by the Swedish expedition, Mossel Bay being closed by ice. The expenses of this

expedition, under the auspices of the Academy of sciences, were defrayed by Mr. O. Smith, a Swedish merchant. It comprised six men, commanded by Mr. Eckholm, who began observations Aug. 23, 1882, and returned Aug. 28 the following year to Tromsø, having carried out the programme without loss or accident.

9. Bossekop, Norway, in latitude $69^{\circ} 54'$, longitude $23^{\circ} 0'$ east of Greenwich. This station, directed by Mr. Steen with four assistants, was established by the Norwegian government under the supervision of the Meteorological office. Observations were carried on during the year beginning Aug. 1, 1882.

10. Sodankylä, Finland, in latitude $67^{\circ} 24'$, and longitude $26^{\circ} 36'$ east of Greenwich. This station was occupied under the auspices of the Finnish scientific society, at the expense of the government, by a party of four observers commanded by Mr. Biese. Observations were carried on from the middle of August, 1882. This station, like the preceding, being situated on the Scandinavian mainland, the position lacked that element of danger inseparable from the navigation of icy seas.

11. Karmakuli station, Moller Bay, Novaia Zemlia. This station, in latitude $72^{\circ} 30'$, longitude $53^{\circ} 0'$ east of Greenwich, was established by Russia under the auspices of the Imperial geographical society, and commanded by Lieut. Andreieff. The international work was carried on, as well as geographical researches. One man died at this station, owing to an amputation of a limb consequent upon an accidental fracture. With the above exception, this party returned safely in September, 1883.

12. Sagastir Island, Lena delta, at the west mouth of the Lena, in latitude $73^{\circ} 0'$, and longitude $124^{\circ} 42'$ east of Greenwich, was the second Russian station. The party under Lieut. Jürgens left St. Petersburg in December, 1881, but did not arrive at its post until the midsummer following. The international programme work was begun in September, 1882; and at the end of the year, all having gone well, the party volunteered for a second year's observations, which should now be about completed. News from the party may be expected in a few months.

13. Dickson Haven, on the north coast of Siberia, near the Yenisei mouth, was to have been occupied by the Dutch expedition, commanded by Professor Snellen of the Meteorological institute, which sailed on the Varna in the summer of 1882. The expedition was beset near Waigat Strait, and was unable to

reach its destination. Observations, except for magnetism, were carried on throughout their stay. The Varna was nipped by the ice, Dec. 24, 1882, but did not sink until the following summer, when the crew and party took refuge on the Dimfna, also beset near by, and later were taken off by the steamer Obi, and reached Hammerfest, Sept. 3, 1883. This was the only expedition which failed to reach the vicinity of the station selected before sailing.

14. In the southern hemisphere, France sent a large party, under Lieut. Courcelle Seneuil, to Orange Harbor, near Cape Horn, in south latitude $55^{\circ} 48'$, longitude $67^{\circ} 30'$ west of Greenwich. Its arrival, successful operations, and return without loss, have already been chronicled in *Science*.

15. Lastly, the German government established on South Georgia, in south latitude 54° , and west longitude 37° , a station under the direction of Dr. C. Schrader. This expedition landed Aug. 21, 1882, and observations were begun early in the following month. It was safely embarked again in the autumn of 1883, without serious accident of any sort, and with the required series of observations, beside large collections in every branch of science.

Beside these extraordinary stations, of whose doings brevity obliges us to give only the barest intimations, nearly all the observatories for magnetism and meteorology in the United States and Europe endeavored to co-operate in the work.

PSYCHIC FORCE.

ALTHOUGH it may be regarded as doubtful whether the society for the investigation of psychic force, proposed at the recent meeting of the American association, will result in any new discoveries, yet the philosophy of the subject is of sufficient interest to merit general consideration. The first and greatest obstacle we meet with in such investigations is the absence of clear ideas of what it is we are to look for, and how we are to distinguish between real relations of cause and effect and mere chance coincidences. The state of mind of the community at large is also unfavorable to the attainment of any result. If we take out of it two classes holding quite opposite views, — the one comprising those who look upon the subject with that sentiment of credulity and wonder which is fatal to all scientific accuracy; and the other, those who think it all nonsense, and unworthy the attention of common-sense peo-

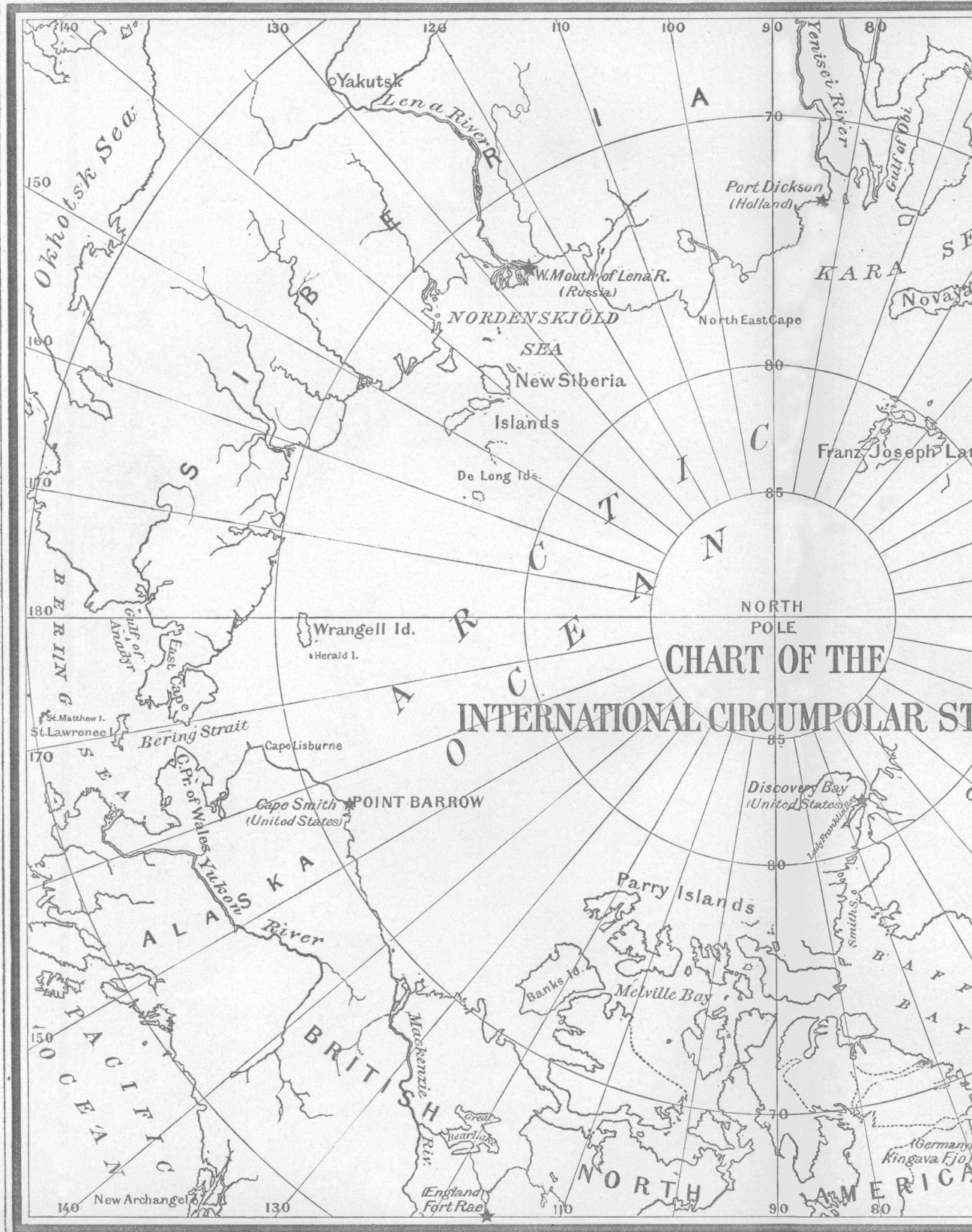
ple, — we shall have but few left for patient research.

If, however, this remnant is going to investigate the subject in a scientific spirit, they are entitled to all the light that can be thrown upon it. We begin by warning them against a kind of inquiry which can lead to absolutely no conclusion. We refer to such inquiries as those made in the following extract in the *New-York Nation* of Aug. 28, 1884: —

Thought-transference, apparitions, etc.

“The Society for psychical research will be grateful for any good evidence bearing on such phenomena as thought-reading, clairvoyance, presentiments, and dreams, noted at the time of occurrence, and afterwards confirmed; unexplained disturbances in places supposed to be haunted; apparitions at the moment of death or otherwise; and of such other abnormal events as may seem to fall under somewhat the same categories.”

It would be difficult for the society to put forth any thing better fitted than this advertisement to lower the estimation in which their work is held by common-sense people. Let us make a little calculation showing how often coincidences of the kind sought for must really occur in our country. Numerical exactness in our data cannot, of course, be reached: all we can do is to make rough estimates which shall not be unreasonably far from the probable truth. Any physician, we apprehend, will consider it quite within the bounds of probability that one per cent of the population of the country are subject to remarkably vivid dreams, illusions, visions, etc. This will make half a million such people in the United States. Each of these persons may be supposed to have fifty friends or relations, of whom one per annum dies. If they are subject to a dream or vision once a week, there is one chance out of seven that they have one on the same day that the friend dies. Let us suppose that it takes a combination of eight separate and independent points of resemblance, between the vision and the circumstances attending the death of the friend, to constitute a remarkable coincidence, and that each of these has a probability of one-half. We shall have, in one case out of two hundred and fifty-six, a remarkable combination of coincidences. Putting these results together, we may infer, that, as a matter of fact, some case of extraordinary coincidence between the circumstances of death, and the dream or vision by a friend of the dying person, does occur somewhere in the country nearly every day in the year. Thus, what the Psychical society will find, will be what we know must exist as the result of chance coinci-



dence. The search after haunted houses is of a different kind, but the result must be equally inconclusive: all that can be discovered is cases in which the cause of some apparently singular phenomena happened to be undiscoverable. The idea seemingly entertained by the psychists—that the residuum, after they have eliminated all cases in which the natural causes could be found, must be genuine—has no logical foundation. One can hardly lie on his bed awake an hour after midnight without hearing some sound the cause of which it is beyond his power to guess; and we do not see any essential distinction between this case and that of a haunted house.

The general question at issue is, whether there is any such process as what the psychists very happily denominate 'telepathy,' which may be defined as *feeling at a distance* without the intervention of any physical agent. And just here we have the real point at issue between them and those people 'of the earth, earthy,' who think their work is all nonsense. The real questions are two in number,—

First, Can the mind be influenced by things external to itself in any other way than by such things acting physically upon the nervous system? Second, Can the mind, by any act of the will, produce any effect outside of itself, except through the agency of the organs of motion of the body itself acting according to physical laws?

The two questions may, perhaps, be combined into one by inquiring whether it is possible that mind can affect mind otherwise than by some physical connection between the nervous systems with which the two minds are associated. That there is a natural tendency to believe in the possibility of the so-called telepathy is, no doubt, well known to all who have considered the subject. The frequently expressed view that the mesmerizer influences his subject by the mere act of his will, and especially the readiness with which this view is received, may be cited as an example. But it is none the less true that the longer we live, the more evidence we see that there is no such action. It is true that this evidence is negative, and so may always lack something of being conclusive; yet the more closely we look into the case, the less foundation we can see for any positive belief in telepathy. We must remember that the physical connection through which one mind affects another may be of the most delicate kind; may, in fact, nearly evade all investigation. The slightest look, an unappreciable motion of the muscles of the mouth or eyes, made perceptible through the light which

is reflected to the eye of the second person, constitute a physical connection. Now, since in the operations of mesmerism the subject is always within easy sight or hearing of the operator, there is always room for the action of a physical cause between the two through the intervention of light or sound. Telepathy between the two could be proved only by finding that the subject was affected by the mesmerizer when the latter was not within sight or hearing or knowledge of the former.

The Society for psychical research has published in its proceedings very detailed accounts of a number of investigations undertaken by its committees and members, some of which are very striking. The report of the committee on haunted houses, however, can hardly be regarded by lookers-on as any thing better than very scientific children's ghost-stories. The extraordinary cases of events or accidents happening to one person being reproduced in the imaginations or visions of others at a distance, are nothing more than recitals of what we know, from the theory of probabilities, must be very frequent occurrences. A feature of these coincidences which ought not to have escaped the notice of the society is, that they have no feature in common by which they can be traced to the action of a general cause, and do not even tend to show that there are particular persons who possess the faculty of being influenced by telepathy. A very striking case is that which most of our readers may have seen, in which a lady awoke under the impression that she had received a blow in the mouth at the very time when her husband, a mile or two away, actually did receive such a blow. Now, if this lady had repeatedly felt her husband's impressions in this way, or if it could be shown that a blow in the mouth or on any other part of the person often makes itself felt by telepathy, the case would be better worth inquiring into; but there is no common feature of this kind in the cases as reported, and they thus fail to supply good evidence that they are any thing more than mere chance coincidences.

The only case that looks at all strong in favor of telepathy is that in which one person is made to draw figures similar to those thought of by another in his neighborhood. If any of the members of our home society can succeed in making this mechanism work, they will have something of great interest to show the critical observer. But we apprehend that the incredulous will, under almost any circumstances, require stronger evidence than any which he has any prospect of getting, to make him

believe that there is no physical cause in action by which the subject has an inkling of the drawings he is to make, or an indication whether he is going right or wrong. This incredulous tendency will be greatly strengthened if the assistance of spiritualistic performers is called in.

S. NEWCOMB.

RADIANT MATTER IN AN EDISON LAMP.

In the Edison exhibit at the Electrical exhibition was shown a phenomenon that deserves careful investigation at the hands of physicists. Midway between the two wires which carry the current to the carbon filament of an ordinary incandescent lamp, a third wire is inserted, which terminates in a thin strip of platinum extending up midway between the branches of the loop with its faces turned towards them, and ending about half an inch below the crown of the loop. When the lamp was in action at its ordinary state of incandescence, if a circuit was closed through a galvanometer between the insulated terminal of the platinum strip and either terminal of the carbon filament, it showed a current flowing across the vacuum of the lamp, between the platinum and the carbon, in opposite directions, according to which pole of the carbon was connected, but much stronger — forty times stronger — when the platinum was connected to the positive pole of the incandescent carbon; this through a galvanometer of about twenty ohms resistance. Moreover, this current was increased when the current through the lamp was increased, so as to heat it much beyond its normal temperature.

After the lamp has been in use for some time, the stronger, positive-platinum, current becomes weaker, and finally changes direction. By letting the lamp rest, the experiment may be repeated. The same currents were obtained *through the glass* when either terminal of the carbon was joined to a small piece of platinum stuck anywhere on the *outside* of the lamp; the same effects were also obtained when the bulb was drawn out into a long tube and the connection made at its end, and when this tube was packed in ice to cool it down; but when the tube was bent round into a loop, no current was obtained, probably from the cutting-off of rectilinear radiation from the carbon.

It would seem as if here were a field for extending Crookes's experiments on radiation.

H. M. PAUL.

THE AMERICAN ORNITHOLOGISTS' UNION.

THE second congress of the American ornithologists' union was held in the American museum of natural history in New York, Sept. 30 and two following days. Dr. Philip Lutley Sclater, Mr. Howard Saunders, and the Rev. E. P. Knubley, of the British ornithologists' union, were present, and took part in the proceedings. A large number of new members were elected.

The report of the committee on the revision of the nomenclature and classification of North-American birds was presented by Dr. Elliott Coues. The work of the committee had been divided; Messrs. Ridgway, Brewster, and Henshaw being charged with determining the status of species and sub-species, while Mr. Allen and Dr. Coues were to formulate the canons of nomenclature and classification. Dr. Coues read at length the report of this last sub-committee, the reading occupying about an hour and a half, after which Mr. Ridgway presented the report of the other sub-committee, which emphatically and unanimously indorsed the employment of trinomials for the designation of sub-species.

The report of the committee on bird-migration was presented by Dr. C. Hart Merriam. This committee had been very industrious, and had been greatly helped by the public press; so that, by the distribution of nearly six thousand circulars, the committee finally secured nearly seven hundred observers, in addition to the keepers of lights. The observers are distributed as follows: Mississippi valley district (Prof. W. W. Cooke, superintendent), 170; New-England district (John H. Sage, superintendent), 142; Atlantic district (Dr. A. K. Fisher, superintendent), 121; Middle-eastern district (Dr. J. M. Wheaton, superintendent), 90; Quebec and the maritime provinces (Montague Chamberlain, superintendent), 56; district of Ontario (Thomas McLlwraith, superintendent), 38; Pacific district (L. Belding, superintendent), 30; Rocky Mountain district (Dr. Edgar A. Mearns, superintendent), 14; Manitoba (Prof. W. W. Cooke, superintendent), 10; British Columbia (John Fannin, superintendent), 5; North-west territories (Ernest E. T. Seton, superintendent), 5; Newfoundland (James P. Howley, superintendent), returns not yet received. Migration-stations now exist in every state and territory in the union, excepting Delaware and Nevada.

The committee was fortunate in obtaining the co-operation of the Department of marine and fisheries of Canada, and of the Lighthouse board of the United States. By this means it secured the free distribution of upwards of twelve hundred sets of schedules and circulars to the keepers of lighthouses, lightships, and beacons, in the United States and British North America.

The returns thus far received from observers were exceedingly voluminous and of great value; they were so extensive, indeed, that it was utterly impossible for the committee to elaborate them without considerable pecuniary aid.

In order to show the union the character and extent of the labors of the committee, the chairman had requested the superintendents of all districts east of the Rocky Mountains to prepare reports upon five common, well-known, and widely distributed birds, — the robin, catbird, Baltimore oriole, purple martin, and nighthawk; and these reports were presented for examination.

The chairman called attention to the action of the International ornithologists' congress held in Vienna last April, stating that he had been instructed (in common with the delegates from other countries) to represent the cause of the committee to the national government, begging it "to further to the utmost the organizing of migration-stations," and "to appropriate a sufficient sum for the support of these stations and for the publication of annual reports of the observations made." The council was instructed to memorialize the U. S. congress, and the parliament of Canada, in behalf of the work of the committee on bird-migration.

The report of the committee on the eligibility or ineligibility of the European house-sparrow in America was presented by Dr. J. B. Holder. Dr. Holder said that a circular of inquiry had been printed, and about a thousand copies circulated in Canada and the United States. Particular pains had been taken to secure evidence from those who advocated the cause of the sparrow. A large number of returns had been received, and the evidence for and against the naturalized exotic had been carefully sifted and summarized. The result overwhelmingly demonstrated that the sum of its injurious qualities far exceeds and cancels the sum of its beneficial qualities: in other words, it was the verdict of the committee that the European house-sparrow is not an eligible bird in North America. The union sustained the decision of the committee.

The report of the committee on faunal areas was presented by Mr. J. A. Allen. Mr. Allen said, that, for the purposes of the committee, North America had been divided into several districts, each of which had been placed in charge of a member of the committee, as follows: arctic and British America and the northern tier of states bordering the Great Lakes, from New York to Minnesota inclusive, were being worked by Dr. C. Hart Merriam; Canada south of the St. Lawrence, and New England, by Mr. Arthur P. Chadbourne; the eastern and middle states from New Jersey to Florida, and west to the Mississippi River, by Dr. A. K. Fisher; the Rocky Mountain region, by Dr. Edgar A. Mearns; and the Pacific region, by Mr. L. Belding. It was the plan of the committee to collate and tabulate the required data from all published sources, to avail itself in like manner of the material contained in the returns of the observers of bird-migration, to illustrate the facts thus obtained by colored maps showing the summer and winter range of each species, and to generalize the final results and place the same before the union, accompanied by colored charts, showing, with as much precision as possible, the exact limits of the several faunal areas in North America.

Dr. P. L. Slater said he was glad to know that North America, which he knew as a nearctic region, was being worked in so thorough a manner by so competent a committee, and that the results obtained could not fail to be of great interest and value.

The matter of the wholesale slaughter of our native birds for millinery and other purposes was brought forcibly before the union by Mr. Brewster, and a committee was appointed for the protection of North-American birds and their eggs against wanton and indiscriminate destruction.

Dr. Leonhard Stejneger exhibited a stuffed specimen of a willow grouse from Newfoundland, which he regarded as a new geographical race, differing from the continental form chiefly in the possession of more or less black upon its primaries. Mr. Brewster said that he had recently examined nearly one hundred and fifty specimens of ptarmigan from Newfoundland, and had observed the peculiarities pointed out, but did not consider them constant. He was inclined to regard the characters mentioned as seasonal, and possibly to some extent individual. Dr. Stejneger replied that this coloration of the wing-feathers could not possibly be seasonal, as they (the primaries) were moulted but once a year. Dr. Merriam stated, that, during a recent visit to Newfoundland, he had examined a very large number of willow grouse in the flesh, and was still engaged in investigating the change of color in this species. His studies led him to disagree with Dr. Stejneger's last statement. Dr. Merriam was convinced that the change in color in individual feathers did take place both independent of and coincident with the moult. Mr. D. G. Elliot agreed with Dr. Merriam in considering the change of color of individual feathers an established fact. An animated discussion followed, and was participated in by many members.

In response to a call from the president, Dr. P. L. Slater said he hoped the members of the union would excuse him if he offended the feelings of any one by the remarks he was about to make. It had grieved him much to find in this country three large and valuable collections of birds which were not under the care of paid, working ornithologists. One of these is in Boston, one in New York, and the third in Philadelphia. Each contains what all ornithologists admit to be most valuable typical specimens. A grave responsibility rests upon the possessors of types of species, and the loss or injury of such specimens is a great and irreparable loss to science. The collection of the Boston society of natural history (known as the LaFrenaye collection) has been much damaged by neglect; and the entire collection ought now to be catalogued, and so arranged as to render any particular specimen readily accessible. In the American museum of natural history in New York are the types of the celebrated Maximilian collection, and many other specimens of exceeding great value. A large number of these have never been properly identified, and some of them are missing and have doubtless been destroyed by insect pests. The value of others has been lost through neglect, by the displacement of labels, and by the omission of

proper measures for their preservation. The same remarks would, in a general way, apply to the collections of the Philadelphia academy of natural science. It is sad to find no paid ornithologists in charge of these exceedingly valuable collections, and he begged to suggest that the union could undertake no worthier task than to impress upon the proper authorities the urgent necessity of immediate action in this matter.

The officers of the union were re-elected as follows: president, J. A. Allen, Cambridge; vice-presidents, Dr. Elliott Coues and Robert Ridgway, Washington; secretary and treasurer, Dr. C. Hart Merriam, Locust Grove, New York.

THE MERIDIAN CONFERENCE.

THE International conference for fixing upon a meridian to be employed as a common zero of longitude met at Washington, Oct. 1, in the diplomatic hall of the State department. Forty delegates were present from twenty-five nations. Of these, sixteen were represented, wholly or in part, by members of the diplomatic or consular service; and, as the State department took charge of the affair, the proceedings have been surrounded with much of the secrecy of that office. As a consequence, the questions involved have been very little discussed from the stand-point of scientific or commercial convenience, but the time has been mostly taken up with political diplomacy and sentiment.

The representatives of this country were Rear-Admiral C. R. P. Rodgers, Messrs. L. M. Rutherford and W. F. Allen, Commander W. T. Sampson, and Professor Cleveland Abbe; and, at the first meeting of the conference, Admiral Rodgers was elected president. In his opening address he referred to the wide extent of this country in longitude, but said there was no desire to urge the choice of a prime meridian within its borders. The rest of the session was occupied in discussing proposed methods of conducting the conference, etc.

At the next meeting, on Oct. 2, Lieut.-Gen. Strachey of Great Britain, Mr. Janssen (director of the observatory at Meudon, France), and Dr. Cruls (director of the Rio Janeiro observatory) were elected secretaries.

Commander Sampson then introduced a resolution to invite the superintendents of the *American ephemeris* and of the Coast and geodetic surveys (Professor Newcomb and Dr. Hilgard), Professor A. Hall, Dr. Valentiner (director of the Karlsruhe observatory), and Sir William Thomson, to attend the meetings. A long discussion arose as to whether these persons were to take part in the proceedings, the French delegates opposing any such proposition. The resolution was finally passed as it stood; and Commander Sampson then introduced another, that the gentlemen who had just been invited to attend the meetings of the conference be permitted to take part in the discussion of all scientific questions. The

French delegates again strongly objected to allowing any private individuals, however eminent, who were not authorized by their respective governments, to influence the decisions of the conference. After considerable discussion, the motion was lost, eight to thirteen, each nation having one vote.

Gen. Strachey then introduced, as a substitute, that the president be authorized, with the concurrence of the delegates, to request an expression of the opinions of the gentlemen invited to attend the conference on any subject on which their opinion might be likely to be valuable; and this was adopted without debate.

Commander Sampson then introduced a resolution that the meetings of the conference be open to interested visitors. This, after objection on the part of the French delegates, was lost by a vote of seven to fourteen.

Mr. Rutherford, in order to give direction and precision to the work, then submitted a resolution that the conference propose to the governments represented the adoption, as a standard meridian, of that of Greenwich, passing through the centre of the transit instrument at the observatory of Greenwich.

The two French delegates made extended remarks opposing such a direct resolution, stating that this conference had no authority definitely to adopt any meridian; that it should not be influenced by the decisions of the geodetic conference last year at Rome, since that was purely a meeting of scientific men on a technical matter, while this conference was more international in its character, and should examine the thing from a political stand-point, Mr. Janssen even going so far as to express the opinion that it should confine its deliberations to the question as to whether a common zero meridian were desirable.

Gen. Strachey said we could not ignore the work of the geodetic conference at Rome; that, composed of some of the most eminent scientific men of all countries, who had fully discussed all these questions, its decisions must carry weight; that while this conference had no authority to enforce its decisions, yet it should make them as complete and definite as possible.

Mr. Rutherford said, that a discussion as to whether it were advisable to adopt a common zero meridian or not was a waste of time; that it was taken for granted by our government in issuing the invitations, and by the others in accepting them, but, out of deference to the wishes of the French delegates, he withdrew the resolution temporarily. Another was then offered by Commander Sampson, stating the desirability of adopting a universal meridian, and it was unanimously agreed to. Mr. Rutherford then renewed his original resolution for the adoption of the Greenwich meridian, and Mr. Janssen reiterated his objections to it.

A discussion followed as to the powers of the conference, and the intentions of this government in calling it. Mr. Rutherford referred to the language of the secretary of state in the invitations, saying that each government was invited "with a view to learning whether its appreciation of the benefits to accrue

to the intimate intercourse of civilized peoples from the consideration and adoption of the suggested common standard of time so far coincides with that of this government as to lead it to accept an invitation to participate in an international conference," etc., and said that they were here to fix upon that meridian; that the delegates must have studied the matter before coming here; and that no one would be likely to come unless he knew, or thought he knew, something about the matter.

Most of the delegates then stated that they had no power to bind or pledge their governments, but only to *recommend* to them the decisions of the conference.

Mr. Fleming, one of the English delegates, called the attention of the conference to the act of congress which called them together; viz.,—

"That the president of the United States be authorized and requested to extend . . . an invitation to appoint delegates . . . *for the purpose of fixing upon a meridian*," etc., and said that the word '*recommend*' was not used at all.

There being, apparently, considerable doubt as to just what they were there for, the conference adjourned over for four days to get further light on the subject.

At the third meeting, on Oct. 6, the pending resolution of Mr. Rutherford was so modified as to define the meridian of Greenwich as a standard meridian *for longitudes*; and it was then temporarily withdrawn to give an opportunity for the French delegates to introduce a resolution providing for a prime meridian having a character of *absolute neutrality*, cutting no great continent.

Gen. Strachey said that the conference at Rome had concluded that a prime meridian must pass through an observatory of the first order; and only those of Berlin, Paris, Greenwich, and Washington fulfilled this condition.

Commander Sampson summarized the many points necessary and desirable in a prime meridian; and, on the side of convenience and economy, he made the strong point, that seventy per cent of all the shipping afloat now use the Greenwich meridian, and that the cost of the plates now engraved for charts reckoning from Greenwich was seventy-five per cent of all the world's charts. To adopt any other meridian would necessitate changing all these, which cost about ten million dollars.

Mr. Rutherford said that the Paris observatory must soon be moved out of the city, and only sentiment kept it where it was; while Greenwich observatory was in an isolated park, secure from injurious encroachment.

Mr. Janssen defended the proposed neutral meridian, saying, that, if that principle were rejected, it would be useless for him to continue the discussion. He went into a long defence of the plan, historical, sentimental, and patriotic; giving the history of the Isle of Ferro as a zero of longitudes, the great work of the French in early days in astronomy, navigation, and chart-making, and how many valuable charts they now possessed, etc. The only point worth combating

was the statement that the needs of the common prime meridian were limited to geography or hydrography alone, and were entirely distinct from the meridians for astronomy, geodesy, and topography, which were local national affairs, and might just as well have separate and independent meridians; in this ignoring the principal objects of the conference.

Professor Adams of Cambridge, England, said that Mr. Janssen's argument seemed to be a defence of the Paris meridian rather than of a neutral meridian, and to be based simply on motives of sentiment and patriotism; that the question of convenience and least change from present status was not touched upon. Besides, why talk of a *neutral* meridian? They were not belligerents, but were all neutral, as scientific men, or men looking for the greatest good to the whole world, should be. If an entirely new meridian be chosen, an observatory must be set up on it, and connected carefully by telegraph with others, and all existing longitudes changed.

Mr. Janssen tried to insist upon the distinction between astronomical and geographical longitudes, and that such a high degree of accuracy was not needed in the latter.

Professor Adams showed that they must, in any case, depend upon astronomical observations; that even geodetic observations of high accuracy cannot determine great differences of longitude exactly, on account of the irregular figure of the earth.

In accordance with a previous resolution, Professor Newcomb, the superintendent of the *American ephemeris*, was invited to give his views on the question. He said it would be impossible to select a meridian absolutely neutral in Mr. Janssen's sense, as it must be on land, with an observatory upon it connected by telegraph with others. He referred to the impossibility of connecting every newly determined longitude directly with the principal meridian, but said that each country or each region must have its secondary meridian and observatory to connect to, and then the whole system would receive systematic correction as the accuracy of determining the longitude of this secondary observatory was increased. He agreed with Professor Adams that the proposals of the French delegates were based purely on sentiment, and that he should answer them just as the former had done.

Gen. Strachey said that longitude was longitude, and as a geographer he must repudiate the idea of first-class longitudes for astronomical purposes and second or third rate geographical longitudes.

At the session of the conference on last Monday the question of a prime meridian was finally settled. Mr. Fleming, the British delegate from Canada, opposed the pending resolution of Mr. Janssen for an absolutely neutral meridian, because it would only add another to those already used, and advocated that of Greenwich on account of the overwhelming preponderance in its present use over any other; while Dr. Cruls of Brazil favored the neutral meridian. The resolution was put to vote, and lost by a large majority.

The original resolution to adopt Greenwich was then introduced. Mr. Allen presented a resolution of

the Railway convention, held in Philadelphia, Oct. 9, and reciting the importance of retaining this meridian. The resolution to adopt Greenwich was then passed with only one dissenting vote, that of San Domingo, France and Brazil not voting.

Mr. Rutherford then introduced a resolution to count longitudes in two directions from Greenwich up to 180°, east longitude being plus, and west minus. This was favored by the delegates from Great Britain and Russia, and opposed by Commander Sampson, the latter advocating the plan of counting only in one direction, from 0° to 360°, as simpler. This plan was also favored by the delegate from Sweden, Count Lewenhaupt, who moved to adopt the fourth resolution of the Roman conference, counting longitude continuously through the whole 360°. Pending further discussion, the conference adjourned till Tuesday at one o'clock. On Tuesday the discussion was continued, and the resolution offered by Mr. Rutherford passed by a small majority.

SEMITIC NOTES.

AN interesting collection of oriental antiquities has been brought to this country by Mr. Bernhard Maimon. The collection consists of bronzes, lamps, manuscripts, seals, and an Assyrian barrel-cylinder with inscription. Mr. Maimon offered it for sale at one thousand dollars, but, finding no purchaser for the whole, he leaves the seals and cylinder in the Metropolitan museum in New York, and has sold the other objects to Professor Marquand of Princeton, N.J.

Information dated London, Sept. 28, has been received, that Dr. W. H. Ward, the leader of the Wolfe expedition to Chaldaea, would set out the following week for Constantinople. Here he hopes to be joined by Dr. Sterrett, who has returned to Constantinople from his extensive tour in Asia Minor. From Constantinople the party will perhaps go by Alexandretta, Aleppo, and Mosul, reaching Bagdad toward the close of November. The months of December, January, and February are those most favorable for a visit to Chaldaea; and the Wolfe party expects during this time to accomplish its task. During his stay in London, preparatory to his trip to Chaldaea, Dr. Ward spent his time in the British museum, studying the Assyrian antiquities, and specially acquainting himself with those which are forged. Cylinders are so valuable, that a flourishing business is done in forgeries by some of the enterprising orientals; but the practical eye can always detect traces of the forgery. Usually a mould is made from a genuine cylinder, and the forgery is cast in this mould. The joining of the two halves of the cast cannot be successfully concealed.

Mr. J. R. Jewett, who graduated at Harvard last year, is now in Beyrout, Syria, engaged in the study of modern Arabic. His favorite studies during his last two college-years were the Semitic languages.

D. G. LYON.

TURNER'S SAMOA.

Samoa a hundred years ago, and long before, together with notes on the cults and customs of twenty-three other islands in the Pacific. By GEORGE TURNER, LL.D., of the London missionary society; with a preface by E. B. TYLOR, F.R.S. London, Macmillan, 1884. 16+395 p. 12°.

THIS work was prepared under very exceptional circumstances favorable to its value and accuracy. The author published, in 1861, a volume entitled 'Nineteen years in Polynesia,' which was chiefly directed to narrate the introduction of Christianity into, and the missionary work in, the group of volcanic islands in Central Polynesia, long known as Navigator's Islands, but correctly called Samoa. In the present volume he abandons the missionary style, as well as its subject, and gives the result of his miscellaneous researches for upwards of forty years. He has clearly apprehended the desiderata in the presentation of the results of ethnological research: i.e., he has confined himself almost exclusively to the detail of facts, classified so as to assist students, but has left to specialists all promulgation or advocacy of theories. The result is that very few works are of greater value in assisting the study of comparative ethnology, or in the solution of problems in physiology, mythology, history, and philology.

The volume, being a repository of an immense number of details in all branches of anthropology, affords little opportunity for such quotation as would give any true idea of its value. It must rather be regarded as a brief encyclopedia of the various titles to which the sociologist, the linguist, the student of folk-lore, the physiologist, and indeed all persons interested in the several divisions of anthropology, can turn with profit. The mythic traditions and the folk-lore constitute, to the general reader, perhaps the most attractive part of the work. In this connection it may be proper to offer a slight criticism.

In the cosmical genealogy, an early character is called 'Valevalenaoa,' or, as translated, 'Space.' This deity had a long-legged seat; and, after a time, 'Cloudy Heavens' brought forth a head, which fell from the heavens. 'Space' set it up on his high stool, and said to it, 'Be a son, be a second with me on the earth.' Space started back, for all of a sudden the body of a man-child was added to the head. The child was sensible, and inquired who his father was. Space replied, "Your father is yonder in the east, yonder in the west, yonder towards the sea, yonder in the land,

yonder above, and yonder below." Then the boy said, "I have found my name: call me 'All the sides of heaven.'" The point for criticism is, that, while the name the boy bestowed upon himself is strictly in accordance with the philosophic status which the Samoan (as well described by the author) had reached, the name or title 'Space' is wholly inappropriate to that status.

What may be the proper translation of the native word 'Valevalenao,' or whether it can be translated, it is not possible for us to determine; but it does seem clear that the metaphysical conception of 'space' could not have been made by the Samoans.

The genealogical table of the divinity gives 'Tangaloa, the explorer of lands,' as his father, and the 'Queen of earth' as his mother; and 'Tangaloa, the explorer of lands,' was the progeny of 'Tangaloa, the dweller of lands,' as his father, and 'Cloudy Heavens' as his mother; also the parents of 'Tangaloa, the dweller of lands,' were 'Cloudless Heavens' for father, and the 'Eighth Heavens' for mother. After that amount of definiteness, it would not be probable that in an attempt to commence from the first of all, Leai (nothing), and arriving at what might be called the practical account of the earth itself, and its deities, one would be constantly encountered with the conception of 'Space' as the progeny of the foregoing. It is true, that, from a metaphysical point of view, space might as well proceed out of nothing, as nothing out of space; but with the intermediaries mentioned, it would not be in accordance with the general lines of savage cosmogony to have started with nothing, and through a respectably elaborate family tree to have arrived at practically the point of departure.

An instance of light is thrown upon a problem which has for some time occupied physiologists. We refer to the subject of prehistoric trephining as explained by an account of the manner in which headache was cured, confirming the theory of Dr. Fletcher in his address before the anthropological society of Washington in 1881, that the prehistoric trephining was to relieve disease of the brain. The operation was to let out the pain at the crown of the head by the following surgery. The scalp was slipped up and folded over, and the cranial bone scraped with a fine-edged shell until the dura mater was reached. Very little blood was allowed to escape. In some cases the scraped aperture was covered over with a thin piece of cocoanut-shell; in other instances the incised scalp was simply replaced.

This is perhaps the first instance in which savage trephiners have been caught in the act with operations on the scale of a custom. The cure was death to some, but most of the cases recovered. To such an extent was this remedy for headache carried on, that sharp-pointed clubs were specially made for the purpose of striking that known weak part of the crown of the head, causing instant death.

The precise operation of trephining has not been found to be practised among the tribes of North America; but they very generally scarified and wounded parts of the body where pain was seated, or supposed so to be. Their philosophy of pain was, that it was an evil spirit which they must let out. The early writers, who believed in the benefits of phlebotomy more than is now the fashion, gave much credit to the Indians for this practice. It was one of the proofs of their advance in medical and surgical science. It is suggested that the custom of cutting the breast, arms, and some other parts of the body, at the mourning ceremonies, may have originated in the idea of letting grief, the pain of sorrow, out of the mourner.

The principles of the taboo are made very clear and expressive by the tale of the devices by which property was protected. For instance, to protect the bread-fruits, the owner would plait some cocoanut leaflets in the form of a sea-pike, and suspend it from one or more of the trees which he wished to protect. The thief would be frightened from touching the tree; expecting, the next time he went to the sea, a sea-pike would dart up, and mortally wound him. Another of the instances is the cross-stick taboo, a piece of any sort of stick suspended horizontally from the tree, expressing the imprecation of the owner that any thief touching it might have a disease running right across his body and remaining fixed there until he died. This is recommended as a contribution to the literature on the mysticisms of the cross.

The interesting subject of tattoo marks is also dwelt on with more than usual information. Reference is made to the mistake of Behrens in describing the natives of Samoa in his narrative of 1772, when he stated that "they were clothed from the waist downward with fringes and a kind of silken stuff, artificially wrought." A nearer inspection would have shown him that the fringes were a bunch of red leaves glistening with cocoanut-oil; and the kind of silken stuff, the elaborate tattooing. An interesting point is the worship of the octopus, or cuttle-fish, which may be compared with its

frequent appearance in the tattoo marks and religious customs of the Haida and other Indians of the north-west coast of America.

The author, not confining himself to the group of the Samoan islands in his forty years' experience, made notes upon the cults and customs of twenty-three other islands in the Pacific Ocean, which are published in this volume. Among these, with reference to the island Nukufetau, is found a singular reversal of the premium on families given by Roman law, and the merit generally attributed, in communities untaught by Malthus, to the production of numerous offspring. Infanticide there was the law of the land. Only one child was allowed to a family. Under special circumstances, and by paying a fine, a second might be allowed to live.

On the whole, and in general terms, without further attempt at quotation, the volume can be strongly recommended as being illustrative of the stage of ethnic life comprehended in it, and as almost above criticism.

THE HOME RAMBLES OF AN AMERICAN NATURALIST.

A naturalist's rambles about home. By C. C. ABBOTT. New York, Appleton, 1884. 485 p. 12°.

It is not often that one can sit down and become so absorbed in a book that he ceases to be critical. It is in this condition that we lay down Dr. Abbott's charming volume. We do not know whether some of his statements need qualifying or not. We do know, however, that the author is an accurate observer, and, furthermore, that he lives amid the scenes and experiences so graphically described. The three beeches, woodshed, fences, etc., do exist, and belong to Dr. Abbott's homestead. The author has been known to the reading public for many years by his articles in the *Popular science monthly*, *American naturalist*, and *Science*. He is more widely known by his being the first to discover paleolithic implements in North America, and as the author of the work entitled 'Primitive industry.'

The present book is, as the title indicates, the rambles of a naturalist about home. The sights and scenes are so well depicted with pen that illustrations are not needed, and the author has had the good sense not to attempt them. Nothing but a sensitive-plate, timed to the fraction of a second, would be of any use in such service. Speaking of a white weasel, he says, 'It fell into the hands of a taxidermist, and was lost to science.' Such a fate often

awaits the exploits he describes when they fall into the hands of an artist.

Many new and interesting facts are given concerning the habits of wild animals, and at the same time he corrects a host of erroneous observations that have gone unchallenged for many years, because no one competent for the work has given the time and patience necessary to the study. His glimpses of wildcats, and the fight between a turtle and mink, are curious experiences, and his observations of the skunk are extremely interesting. He alludes to the peculiar power of the skunk as causing an 'atmospheric disturbance'! The rapidity with which a skunk burrows in the ground is quite a new fact. He shows how untrustworthy most weather-lore is, as based on the habits of animals, though he admits that chipmunks appear to foresee the occurrence of a cold rain twenty-four hours in advance. He also shows—it seems to us conclusively—that the opossum does not 'play possum,' and that its supposed power of feigning death is the result of paralysis from fear.

He believes that the gambols and antics and various curious behaviors of animals are evidences of play and fun, as in children, and that in no other way can such behavior be explained. Even among fishes has he observed movements that must be referable to the same desire. We can commend the book most heartily to all lovers of nature. It is a book to be put into the hands of every boy, and we should like to see it adopted in our schools as an occasional reading-book.

THE LIFE OF ELLEN WATSON.

A record of Ellen Watson. Arranged and edited by ANNA BUCKLAND. London, Macmillan, 1884. 6+279 p. 8°.

ELLEN WATSON's claim to remembrance does not rest upon what she did, but upon the promise she gave of what she might have done had her life been longer. At the age of twenty she entered University college as the first woman-student in mathematics and physics. Professor Clifford soon formed a very high opinion of her mathematical ability, and believed that she possessed a rare faculty for original work. In the examination which was held at the end of the year, he was careful not to allow his judgment to be influenced by the fact of her youth and sex; and the most strict examination of her papers gave her the highest number of marks gained by any of the class, and placed her in the position of first

mathematical student for that year in University college. She was awarded the principal prize in applied mathematics and mechanics, and the Mayer de Rothschild exhibition; and Professor Clifford said, at the meeting for the distribution of prizes, that a few more students like Miss Watson would certainly raise University college to a status surpassing that of institutions twenty times as rich, and which had been two hundred years longer in existence. Praise like this from Professor Clifford would have been remarkable if it had followed years of preparation under such skilful training as English tutors know how to give. Ellen Watson had not only carried on her studies by herself, but she had been from the age of sixteen the governess, the playfellow, the nurse, of a large family of younger brothers and sisters. In order to get a little uninterrupted time for the study of quaternions and the calculus of variations, she had been obliged to form the plan of going to bed with the children, and getting up at four o'clock in the morning to begin her day's work. Such success, under such circumstances, gives reason to believe, that, if she had lived, she would have been one of the most remarkable women of her time. Her disease was consumption; and it does not appear that her death, at the age of twenty-four, was hastened by overwork. No less remarkable than her intellectual ability were the sweetness and elevation of her character. Her later correspondence shows a lofty aspiration, a passion for some high undertaking for the good of the world which her early death prevented her from entering upon. Great minds of either sex are not so common that one can feel less than profound regret that one more has been extinguished without great work accomplished.

NOTES AND NEWS.

THE parental relation of the large cyclonic areas of low pressure that frequently pass over our country, and which might well be called simply *cyclones*, to the tornadoes that are formed in them, has lately been discussed by W. M. Davis in the *American meteorological journal* for August; and by H. A. Hazen in the same, and in the *American journal of science* for September. The former gives a graphic illustration of about one hundred tornadoes that occurred last spring, according to Lieut. Finley's maps; the latter gives a tabular statement of a number of tornadoes of earlier years. The results agree in showing the close limitation of tornadoes to a district south-south-east of cyclone centres, as has already been pointed out in these notes; but the authors differ as to the theoretical meaning of this limitation.

— Professor Simon Newcomb, LL.D., superintendent of the U. S. nautical almanac, has been appointed professor of mathematics and astronomy in the Johns Hopkins university.

— The comet discovered by Wolf at Heidelberg, on Sept. 17, proves to belong to the interesting family of periodical comets, according to the calculations made at the Harvard college observatory by Mr. S. C. Chandler, jun., and Mr. Wendell. An attempt was made to compute an orbit from observations, Sept. 20, Oct. 1, and Oct. 11; but it was found that they could not be represented within several minutes of arc on the assumption of parabolic motion. The parabola obtained was, perihelion passage, 1884, Nov. 14, 23,309, Greenwich mean time; perihelion from node, $170^{\circ} 40' 36''.0$, 1884.0; node, $197^{\circ} 16' 24''.3$, 1884.0; inclination, $34^{\circ} 0' 46''.8$, 1884.0; log. perihelion distance, 0.273507; which gave the deviation of the middle place ($C - O$), $\Delta \lambda \cos \beta = + 7' 35''.8$, $\Delta \beta = + 4' 40''.5$. These residuals could not be sensibly reduced by varying the ratio of the extreme curtate distances. Accordingly an orbit was computed without any assumption as to the form, with the following result: perihelion passage, 1884, Nov. 17, 71,070, Greenwich mean time; perihelion from node, $172^{\circ} 36' 40''.5$; node, $206^{\circ} 27' 36''.5$; inclination, $25^{\circ} 10' 54''.3$; log. perihelion distance, 0.196049; mean distance, 3.53638; eccentricity, 0.555885. The corresponding period is 2,429 days, or about 6.65 years.

This comet accordingly appears to belong to the group of the Faye-Möller comet, 1857, iv., and 1874, iv., all of which have general features of resemblance. There is no evidence of any known previous appearance of this comet. If, indeed, the period above given is not considerably in error, it would be visible from the earth only at every third return to perihelion, or once in twenty years.

— Dr. Charles Rau, curator of antiquities in the U. S. national museum, Washington, D.C., is about to publish, under the auspices of the Smithsonian institution, a most valuable and interesting work entitled 'Prehistoric fishing in Europe and North America.' This work will form No. 509 of 'Smithsonian contributions to knowledge,' and consists of about 350 pages quarto. The book is illustrated with four hundred and five cuts from drawings by Mr. Trill, being either copies of already published designs, or correct representations of objects specially drawn for this work, the majority of the latter being specimens belonging to the U. S. national museum. As regards America, objects termed 'prehistoric' include such as are found in mounds and other ancient burial-places, on and below the ground, or in caves, shell-heaps, etc.; in fact, to use Dr. Rau's words, "all articles of aboriginal workmanship, that cannot with certainty be ascribed to any of the tribes which are still in existence, or have become extinct within historical times, or, to speak more distinctly, within the recollection of the white successors of the Indians."

This book is divided into two parts: part i. Europe; part ii. North America. Part i. is divided into three sections: 1°. Paleolithic age, 2°. Neolithic age, 3°. Bronze age. In part i., Europe, a short characteri-

zation of the three ages is presented, followed by a minute description of the fishing-implements peculiar to each period. Special attention is given to the fishing-articles found in the paleolithic caves of France and other countries. In the neolithic age, the artificial shell deposits of Denmark and of the other lake-dwellings of Europe are especially noticed. In the bronze age are considered the fishing-implements from the lake-settlements of Europe, and also those forms of implements not found in lake-dwellings. In part ii., North America, the subject is discussed under the following chief headings: (a) fishing-implements and utensils, (b) boats and appurtenances, (c) prehistoric structures connected with fishing, (d) representations of fishes, aquatic mammals, etc.; and (e) artificial shell-heaps. Then follow extracts from various writings of the sixteenth, seventeenth, eighteenth, and nineteenth centuries, in which reference is made to aboriginal fishing in North America; the work closing with notices of fishing-implements and fish representations discovered south of Mexico. Plate proof of this work has already been furnished to the author, and it is probable that in a few weeks this book will be within the reach of archeologists and others.

—Capt. James Mercer has been placed at the head of the department of civil and military engineering at West Point in place of Prof. Junius B. Wheeler, retired.

—The navy department has ordered Assistant Engineer Gould H. Bull, U.S.N., to Philadelphia, as professor of engineering at the University of Pennsylvania.

—The work of establishing cold-wave flag-stations is being carried on under the supervision of First Lieut. Dunwoody, acting signal-officer. He has sent out over eight thousand circulars to postmasters in the cold-wave sections, answers to which are being received daily; and there is every reason to think that within two months the flag system will be in working order. The warnings regarding cold waves will be transmitted by telegraph to the sixteen signal-corps printing-stations in the north, east, and west, whence copies of the *Farmer's bulletin* will be sent out to the different stations in the vicinity of each printing-office; and the postmaster receiving the warning will hoist his flag, thus giving notice to his neighbors of the near approach of a cold snap. These flags are not to be hauled down until twenty-four hours after the postmaster receives notice through the *Bulletin*, and then only in case he does not receive a second warning.

—Prof. J. W. Mallet of the University of Virginia has accepted the position of professor of chemistry in the Jefferson medical college, Philadelphia, which has been recently held by Prof. Robert E. Rogers.

—William C. Day, Ph.D., formerly of St. John's college, Maryland, has been appointed professor of chemistry and physics at Nashville university, Tennessee.

—The geological maps of America are excellently catalogued by J. and J. B. Marcou in their 'Mapoteca

geologica Americana,' just issued as a bulletin of the U. S. geological survey. They reach the surprising number of 924; including, however, under distinct numbers, all re-issues. The annotations are brief but valuable; and the whole is prefixed by a very interesting account of the progress of geological cartography, which is of permanent value. An excellent index completes a most serviceable publication.

—Abram S. Hall, Ph.D., a graduate of the University of Michigan, is appointed professor of chemistry and physics at St. John's college, Annapolis, Md.

—There is an error in the reports of the committees of the American association, in our last issue, which needs correcting. The statements concerning the committee on an international convention refer, not to that committee, but to the committee on the interchange of courtesies between the American and British associations for the advancement of science.

—Mr. J. Dickie of Leeds is exhibiting his recently patented invention of an aqua-aërial or wave-ship, which is supposed to be capable of making the channel passage in twenty minutes, or of running to New York and back in six days. The aqua-aërial ship presents a different section at different parts of its length; but it may be described as a broad, flat vessel with water-tight chambers all round it, and a series of three inclined planes forming the bottom. The air-ducts are of the usual shape on deck, but spread out so as to occupy one-half the breadth of the vessel at the point where they reach the bottom. They are situated just at the commencement of the inclined planes, and as two are placed side by side there are four altogether. The object of these ducts is to render each plane independent of the others; and thus all are supposed to assist in lifting the vessel out of the water, as it were, and to facilitate its passage over the surface. The bows curve downwards from about the deck level, and merge into the front of the first plane of the bottom; while the water-tight compartments at the sides of the vessel are formed into a sort of platform at the stern, by means of which eddy-making is to be avoided. The air-ducts have another office to perform; for, by means of self-acting valves, any tendency to roll is said to be immediately counteracted by the air-ducts on the rising side of the vessel closing automatically, thus creating a vacuum on that side, while the greater pressure exerted on the water on the other side will tend to restore it to the normal level. The inventor maintains that the power required to keep up the speed will decrease with the increase of the vessel's rate of progression, 'the only thing necessary being a high speed of engines.' Unfortunately for sea-sick people, we have as yet had no practical proof of the merits of the 'aqua-aërial vessel.'

—A cable despatch was received Oct. 15, at Harvard college observatory, from Kiel, Germany, announcing the discovery of another asteroid by Palisa. Its position was as follows: Oct. 14, 4033; right ascension, 2^h 18^m 26.3^s; declination, north 13° 47'; daily motion, west 56'', south 6'. It is of the 13th magnitude.

SCIENCE.

SUPPLEMENT TO No. 89, FRIDAY, OCTOBER 17, 1884.

THE DISCUSSION ON STORAGE-BATTERIES BEFORE THE ELECTRICAL CONFERENCE IN PHILADELPHIA.

THE sudden outburst of interest in secondary batteries at the time the Faure battery was first introduced in England led to some unfortunate results in the shape of bankrupt electric companies. The first hope was that sufficient electricity to do most of the household work, and give all needed light, would be left at one's door each morning. The idea of 'bottled electricity' was a taking one with the public, especially as it was introduced with the best of endorsements; but the secondary battery has not proved to be such a boon as was expected. In view of this great public interest, the discussion on the subject by many of the leading electricians of England and America at the electrical conference was of especial importance. This discussion shows, as well as any thing can, the present state of opinion among those most capable of judging of the secondary battery. The discussion was opened by Mr. W. H. Preece.

Professor W. H. PREECE, London, England. — I have been called upon somewhat unexpectedly to open a discussion on this question of storage-batteries; and I regret very much that I have no notes with me to refer to, and that I shall have to trust a good deal to my memory. As I am not possessed of a poor memory, I don't think I should develop any serious error, although Mr. Edison himself has declared that this question of storage-batteries has developed the most remarkable power of man's latent capacity for lying. [Laughter.]

I very much agree with Mr. Edison's definition, for I think there has been more lying and more swindling and more rascality done over this question of storage-batteries than over any other department of electrical science. Now, storage-batteries have been very improperly called storage-batteries, and I very much prefer to call them secondary batteries. It is quite true that this subject of storage-batteries is one upon which there is a good deal of misconception, which has arisen from the introduction of the word 'storage.' Now, these secondary batteries have been before the world for over twenty years; and all the physicists who go to Paris almost invariably meet with Planté himself, the father of this instrument. For the past twenty years, nearly, it has been one of my great pleasures, when I visited Paris, to pay a visit to Mr. Planté, who has shown me, as he always shows everybody who sees him, the progress that he has made. Mr. Camille

Faure, conceived the notion of coating the plates of lead with the oxide of lead. These secondary batteries had not attracted much attention, although Planté's papers deserve the most careful consideration from all electricians; but Mr. Faure found that by coating the positive plate of the secondary cell with a layer of minium, — red oxide of lead, — he considerably hastened the production of the plate. Planté simply depended upon the electrolytic action of the current in peroxidizing his plate. This is an operation that sometimes involves months. Faure's battery fell into the hands of a man who put forward one of the most diabolical schemes that Paris has ever produced. A man by the name of Phillippart placarded all Paris with the most outrageous notices of what this great battery was going to do. Batteries were to be distributed, like milk and ice, at our doors; so that motor power could be obtained and used in our houses, and light could be obtained from them. The result was, that a considerable sensation was created, and an attempt was made to bring out an official swindle; but it did not succeed. Another gentleman was associated with him by the name of Mari. They became associated in London with a gentleman who holds a very high position, and occupies in the financial world a very important place indeed, as the head of the great firm of Mathie, Johnson, & Co., who are large metallurgists. Mr. Sellon joined himself with a Mr. Volckmar. They brought out a compound battery; and a company was formed in London to develop this scheme, and a great deal of money was collected, but all of it has been lost. Mr. Volckmar himself succeeded in playing his part of a plunderer sufficiently to ride about London in a manner indicating a person of great wealth. The fate of this man was sad indeed, for his body was found in the Seine with a bullet through the forehead. Now, two or three other men had been working at this idea, — Mr. Tribe and Professor Bridgeton. They approached the matter in a truly scientific spirit; and the result has been, that Mr. Tribe has brought out a form of secondary cell, although it is not yet in the market, that is a great improvement upon any thing that has ever been brought out before. But all these persons have been working silently upon this subject, and have gone back to Planté's original work, and they have followed Planté's original methods. The batteries that I am going to speak about are hollow,

and the modification of Planté's simple lead cells; the simple cell being two plates placed in a dilute sulphuric acid, one plate oxidized, and the other plate pure. Now, as regards the uses, I have made a great many experiments with these secondary batteries; and first I will speak of the experiments made of their use in telegraphy. Some time ago the idea was mooted that a considerable economy might be effected in the working telegraph if dynamos were used for that purpose. The dynamo-machines are used by the Western union company, and they employ them for their circuits; but they are not used in England, and we have failed to use them for a very obvious reason, which is simply this, that in England we do not work with 'closed circuits.' We work our instruments with rapid reversals; our automatic system, which is so very extensive, sometimes giving us the result of the transmission of three hundred words per minute, with very rapid reversals of very minute currents. It is absolutely essential that these currents should be uniform in their character. Now, the currents produced by the dynamo-machines are not uniform. If a telephone be inserted in a secondary circuit, you will invariably find that you can hear sounds which are indicative of variations of the current, and these variations are fatal to a fast speed of working. But when you use a dynamo, and utilize the current of the dynamo in preparing a secondary battery, you then get a means by which you can produce currents of absolute uniformity. The secondary-battery current flows out absolutely uniform. And, again, it has the great advantage of giving you a battery with a very low resistance. The electromotive force of an ordinary cell is as low as two volts. You may take it as a rule to be two volts. Its internal resistance may be made whatever you like. Now, I had three series of cells made, one set being Dr. Tribe's, and another Planté's: and from each of these sets — there were eight cells to each set — from each of these sets forty circuits were worked; that is, each battery had forty distinct and separate circuits, so that practically we had a hundred and twenty circuits running from these three secondary batteries. Although all worked for about three months, I think the exact time was ninety-six days, all these circuits worked uniformly and perfectly, gave no trouble that would necessitate even the glancing at them during this whole period of three months, and without any attention; when suddenly one failed, and immediately afterward another failed, and then the third failed. They were charged up again, and then they went on for another three months. And they have behaved as well as one could possibly wish; so much so, that one of the first duties that I wish to discharge, on my return to England, will be to arrange for a large supply of these secondary cells, and a further use on a large scale at our battery for the post-office. We have in use, at the general post-office in London, six hundred and fifty circuits centralizing there; and we are now utilizing about twenty-two thousand cells. I think it more than likely we shall be able to work the whole with probably not more than five thousand cells.

Now, as regards electric lighting. I have already used secondary batteries for electric lighting: I have used them in the post-office, and I have used them for my own house. My house is in a portion of the country through which no lines pass for the purpose of electric lighting, and they cannot be found within a reasonable distance: so, if I wanted to light my house by electricity, I must be dependent on my own supply of electricity. I light my house by gas: but I burn my gas in my garden, and I extract from it that which I want, namely, light; and I discharge into the air of my garden that which I do not want, namely, poison. My gas is employed in working a small gas-engine of two-horse power. It is the gas-engine which works a small Gramme dynamo, which, when worked at its full power, gives me forty-two volts and fifty-two amperes. These fifty-two amperes are directed into seventeen cells. The cells are Planté's original cells. The plates themselves are two feet square, and in each cell there are twelve of these little lead plates. The lead plates are made up of four thin sheets of lead, each sheet being about one thirty-second of an inch thick; and they are perforated in squares regularly all over, and these four thin plates are tied together — they are almost woven together — they are tied together with thick worsted. They are arranged in pairs; six on one side forming one pole, and six on the other side forming the other pole. They are placed inside of an ordinary pitch-pine box, and the insulation of the cell is maintained by a thin India-rubber bag which envelops it. It is a loose India-rubber bag of about the same shape, and, when the plates are put in, it forces the India-rubber out, and the bag takes the exact form of the cell, and it makes the cell thoroughly water tight and thoroughly electricity tight. I have seventeen of these cells, all arranged in a series; and my engine, which generates these currents, and charges this battery, is in my garden. My gardener has the gas-engine under his charge. He is an ordinarily intelligent gardener. When he comes on duty in the morning, he lights the gas in the gas-engine, and starts the dynamo which charges my battery; and when he goes away to his dinner, after the engine has been working for three or four hours, the battery is prepared. I go home in the evening: I have got a store of electricity. I have every room in my house fitted up. I have at my bedside a most charming little light, with no smoking, and no trouble with a wick, and no heat, — a mellow light by merely turning on my tap. I have the softest and most delicious light you can conceive of, thrown upon my paper or thrown upon my book. I have had this going on for four months, and I have never had any bother, except on one occasion, when the gardener put his foot on the wrong place, and the engine came down upon his foot, crushing his toes.

I have a strong impression, that, for all isolated places similar to mine, the storage-battery is an essential thing, and I think that some one is bound to work out this question. Several are now pegging away at it. Sir William Thomson spent much time in investigating this subject, and has given it a great deal of attention, but has not succeeded, simply because you can use a Planté cell untrammelled by any patent. It

is at your disposal, and nobody will interfere with you. If anybody else chooses to follow my example, I think they can do precisely the same thing. But there are, after all, one or two other uses for secondary batteries. Now, I have been very luxurious and very extravagant in carrying out this arrangement. For instance: I have a little daughter who has a very pretty doll-house. Her doll-house consists of six rooms; and each room is well furnished, and well populated with charming little dolls. It is, of course, necessary that each room should be supplied with a charming little electric light: so each room is supplied with this light. Then, I have a cigarette-lighter, consisting of a piece of platinum which can be rendered red-hot by the secondary battery. The difficulty in doing that sort of thing with the usual mode of distribution is that you want the benefit of cutting off a portion of your current. I interrupt a current, flowing, say, six-tenths of an ampère, by putting into the circuit, by a switch, a secondary cell, that diminishes the light, and sets up a counter-electromotive force of two volts for the time being. While that cell is in the circuit, your light is a little dim; but when I take from the poles of that secondary circuit two wires in connection with a piece of platinum wire, then the current passes through the platinum; and this current is due to the two volts from the secondary cell. This piece of platinum wire is heated up with a current quite sufficient for the purpose, with only two volts.

Now, there is another field in which satisfactory experiments have been made with secondary cells, and that is, in lighting up trains of cars by electricity. This has been done on one of our railways between London and Brighton. The railway company has been for some months past lighting up one of its express trains with secondary batteries. The dynamo is worked by the motion of the wheel, and the dynamo charges the battery during the time that the train is working, and the battery is being charged during the whole of that time, and the current is being extracted from the battery; so that you have a light in your carriages which is perfectly steady, and quite independent of the motion of the train. And that leads me to a point which I omitted, and which led me to work so hard at secondary batteries; and that is, that a secondary battery renders the current produced by the inconstant engine and the inconstant dynamo perfectly steady.

It is a difficult thing to say, that, in making an appliance of any kind, we have reached absolute perfection; but I can say this, that, when you have a secondary battery inserted in the electric-light circuit as a shunt (p. 388), it renders your current perfectly uniform, and your light is as near perfection as it can be. Supposing that your lamp requires fifty volts, then you will require twenty-five cells; and I should think myself that it would be quite possible to make cells for this purpose that ought not to cost more than five shillings a cell. You put twenty-five of these in cells: the first current that goes through, simply charges the lead plates; one is coated with oxide, and the other is clear; and you get a counter-

electromotive force of fifty volts. The consequence is, no current whatever passes through the secondary cell unless it has once been raised to fifty volts. The whole current goes through your lamp unless there is flickering of the engine. Then, if there is flickering of the engine, the energy that is stored up in the battery passing through the lamps makes the light uniform, and in that way you get the storage effect by the use of the secondary battery.

There are certain defects that have developed themselves in these batteries, that have been gradually cured. The great defect in all secondary batteries is due to buckling,—a fact due to the formation of peroxide upon the plate. You will find your lead plate will buckle up into all kinds of positions, and the two plates will come into contact. This difficulty I have sought to overcome by supporting the two plates by means of a plate made of paraffine-wood; but the most effective arrangement that I have tried is ebonite. This ebonite is furnished, stamped out to the proper size, and is very light and very thin, though quite equal to preventing all buckling.

Secondary cells have not been in use long enough to enable us to determine how long they will last. I have had them in use for four months, with a sign of but little disintegration of the positive plate. I think that the plates will have to be renewed only about once in every two years. Those that I have in use were re-charged at the end of four months.

The charm of the whole thing is such, and its cost is so trifling, that I shall keep it in my house; and I am quite sure that all those who have worked in the same direction in regard to this matter of electric lighting will never give it up. The electric light itself has some sort of a charm about it. Objection is made to the cost of introducing it; but I have protested often and often against the comparison that is drawn between the cost of gas and the cost of electricity. The two things are not to be compared. When we indulge in luxuries, we don't compare the cost of the luxuries with other things. If you want fine 1834 port, you don't compare its price with that of ordinary claret. If you want to indulge in a fine pheasant, you don't compare it with the old cock that crowed before Peter. So, when you have a delightful luxury like electricity, you don't want to compare its cost with that of gas or sperm-candles, or any other mode by which life is shortened and ultimately destroyed. Here we have something that in the out-of-the-way houses tends to lengthen life and to satisfy us, giving us something cool and delicious; and I say, all comparisons with gas are utterly ridiculous. In reference to these enterprising light-companies, they will soon bring electricity to our doors; and we will all take it. When people can get electricity at their doors, and can get it without much cost, as indeed they can, they will certainly have it. So I look upon the days of gas as being numbered; but gas is a most important power, and its uses are just in their infancy. The days of gas as a distributor of power and heat are coming. At the same time, electricity is going to supply light such as we want.

There are many facts connected with the working that I should have liked to give you. I should have liked to give you the efficiency. I have made careful experiments as to the efficiency of the storage up to the present time. The cost is certainly within the reach of every man in this room. Every single man in this room can afford the comfort which this light will give him; and an electric plant will enable him to live four or five years longer in this world than he would without the electric light.

Prof. W. H. HARKNESS. — I was very much pleased and gratified at hearing Mr. Preece's remark as to the use of secondary batteries. After all that has been said on both sides, it is gratifying to find that we have a certain and valuable method of lighting which is effective and economical. I wish also to add my testimony on one or two points which Mr. Preece has mentioned, and that is, to the courtesy uniformly shown by Mr. Planté to any one who visits him at his laboratory. I had the pleasure, with some friends, of visiting his laboratory, witnessing many experiments made with a large number of secondary-battery cells. As we all very well know, his invention has resulted in the adoption, not only of those secondary cells, but also of the cells themselves. The use to which he has put them, however, has led to many absolutely new experiments, so far as I know. It is also a point of interest to know, as probably some of you do know, that Planté himself tried a method, many years ago, of covering the plates with minium. If Professor Barker were here, he would bear witness to the fact that Planté has used both; that he tried the same thing several years ago, and found that it was not so effective as a battery of his own plates by the method adopted, which, though necessarily slow, probably resulted in a better form of cell. So it is gratifying to know, from the experiments of Professor Preece, that we are advancing, so far as new experiments are concerned, and that we are finally going back to the original form devised by Planté.

There may be one reason worth mentioning. We are all well aware of the careful and interesting experiments referred to by Mr. Bright, on the chemistry of the secondary battery. These experiments show us that there is a greater or less formation of the lead sulphate in connection with the cell adopted; and that also shows us why the electromotive force, having run down, after a period of rest, is recovered to a certain extent. A great many points have been cleared up that were certainly quite enigmatical before. Now, some experiments, and I think some of those that have been published by Professor Barker, go to show, that, in the Planté cell as formed by Planté, — by a rather slow and tedious process, but by the method of Planté, — there is at least a small amount of lead sulphate produced. I take it that the giving-out of the cell is due to the formation of sulphate; so that, if these cells are formed by the rules laid down by Planté, they will have a tolerably long life. Now, just one word with regard to the cost. You will remember that a year or so ago Professor Langley showed, I think beyond any question, that the percentage of the radiant

energy from an Argand gas-burner that is effective in producing illumination is less than one per cent. It certainly is a very curious result. It is less than one per cent, and there is little doubt about the energy which goes up through the chimney.

Mr. N. S. KEITH. — Planté was evidently far ahead of his time. He produced what is to-day found to be of vast practical importance. In 1860 or 1861, about the time of his published experiments, there was no electric lighting; certainly none as it exists to-day. There were then electric-lighting men who were far ahead of their time; but it is only within the past four years, we may say, that we have had any possible application of the secondary battery. I am led very forcibly to consider this point, because in 1878, during the experiment I was then carrying on in the electrolysis of lead, and in collateral experiments which relate to the chemical action of lead in various solvents and in various electrolyses, I was led to make a secondary battery after having made one of Planté's. I made one by coating the plates with peroxide of lead by electro-deposition — not peroxide of lead formed from the substance of the plate itself, as by Planté; not by coating the lead plate with oxide of lead, or peroxide of lead, as the case may be, from external sources; but by coating it by deposition from a solution of lead in which the plates were immersed. I took two hundred and forty cast pieces of lead plate, each one foot square. I divided them into ten cells, making twenty-four plates of each cell, twelve of which were positive, and twelve negative. By using a suitable solution of lead (a sub-acetate of lead in nitrate of sodium), and by treating that with a current of electricity, I produced decomposition of the solution, deposition of peroxide on the positive plate, and at the same time metallic lead was deposited upon the negative plate in a very finely divided and crystalline state. Some hydrogen is also deposited with the lead, — an equivalent to the oxygen that is deposited in chemical combination with the lead upon the positive plate. These plates, after pressing the metallic lead so as to cause it to cohere and adhere to the plate, were taken from this solution and immersed in sulphuric acid, and gave a very satisfactory secondary battery indeed. At that time (in May, 1878) I ran a dynamo as an electric motor with this battery, and I effected some chemical and electrolytic decompositions in a solution of sulphate of copper, and did some other things which were then the only possible commercial applications of secondary batteries. Since that time, electric lighting by incandescence has come to the front, and we find the great use of secondary batteries which has been so forcibly set forth by Professor Preece. At that time we did not have telegraphy by the dynamo; but we had telegraphy by the use of the primary batteries with which we are all so familiar. I have since that time carried on a considerable number of experiments relating to secondary batteries. The whole subject is an electrochemical one. There is a chemical decomposition of the solution by the passage of a current of electricity. There is a decomposition of the water of the electrolyte either primarily or secondarily. Oxygen goes

to one plate, hydrogen to the other. For a new experiment I have taken solutions which suffer decomposition without delivering free hydrogen and oxygen. I have taken, for instance, solutions of the sub-salts of lead. A current causes decomposition, and a deposition upon one plate of peroxide of lead, and, upon the other, of metallic lead. For instance: I have taken, for one plate, a plate of carbon, and, for the other, a plate of lead. A solution of a low sub-salt of lead was used as an electrolyte. By passing a current of electricity through it, the action is to raise the sub-salt to a proto-salt. This takes energy, which is absorbed by the chemical action. Then the charge of electricity is returned until the peroxide of lead has again entered the solutions, and the lead has also entered into the solution. The action of the charging current of electricity is to raise the electrolyte to an acid condition; but the charging action is stopped before that point is reached, because the end would be defeated in passing it. I am also carrying on some experiments in the way of storage for electricity or in the use of secondary batteries. I can hardly give particulars, except this: the idea is to preserve the integrity of the plate—of the inside plate—for an indefinite length of time, say, as long as may be practically worth while. So far, after some months' use, I have been able to oxidize the plate to the desired depth (say, one-sixteenth of an inch), and preserve a backing of metallic lead. Under this change, if my theory and practice be correct, I will not be obliged to replace, as in the present known storage-batteries, the oxide-plate.

Prof. CHARLES H. KOYLE.—I should like to say something in reference to the theory of the secondary battery. There are no substances which, placed in juxtaposition in a secondary battery, will give the electromotive force that the peroxide of lead, and the lead of the Planté cell, laid opposite to each other, will. It remains as it was in 1859, when Planté experimented and wrote. I have experimented upon other substances; and, although my experience has not been exactly useless, it has been the next thing to it.

The necessities of the plate, then, are the only difficulties to be considered. They are three, and they are very simple ones. First, the plate made must be a conductor of electricity, and that leaves us the metals. The plate, in the second place, must resist the ordinary action of sulphuric acid: that leaves us lead and four or five expensive metals,—platinum, and so on, and gold and carbon. Our list is very much reduced, as we have practically, for this purpose, only carbon, as the expense of the other metals is too great to allow them to be used extensively for commercial purposes.

Now, when we get the third and last condition,—and that is, that the plate must resist both electrolytic action and the oxidizing action of the current,—lead will resist the electrolytic action of the current moderately well. But the oxidizing action of the plate reduces the lead to a peroxide in a very short time. That leaves us only one thing for a secondary battery of the future,—carbon.

I think there is no way of upsetting that very short argument. I should be very glad to know if there is. Now, then, the manufacturer of the carbon plates called good, makes plates which shall contain a very large amount of the substance, because the capacity of the storage-battery depends entirely upon the amount of lead and peroxide of lead which you can put upon these plates. They are the substances that you will have to depend upon for your electricity. Of course it is not necessary to say that they will answer the purpose: they are the substances in which the chemical action takes place.

Now, it is possible to take carbon and to manufacture carbon plates by the ordinary method. Take ground charcoal, ground gas-carbon, ground flint, and mix gas-tar with it, or molasses, or any hydrocarbon. This is put into a press and the shape is given to it; and then it is put into a closed furnace and carbonized, so as to drive off the gas, and leave the carbon free. Those plates are pretty good for ordinary primary batteries; but when your lead contains a lot of little holes, very much as the lead now in use does, the plate is very apt to disintegrate. There is such an enormous amount of surface exposed, that the action of the cell is apt to disintegrate the plate, and the metal will scale off. A better way to make the plate is then to be considered. There is a method of making these plates which is very much liked,—a method in use by the U. S. lighting company in the manufacture of carbon for their incandescent lamps; that is, to take celluloid and put it through some chemical operation which deprives it of a part of its gum, thereby leaving a substance very easily carbonized. That substance may be cut into any shape of plates desired. You can make some of these plates thick, and some thin, and you will always have a good plate. It is not very strong, but it is a good plate. Another method, which I myself have used, I have found to work very well, and I shall have more to say about it in the course of a few minutes. At present I can only tell you the general method. I take gas-tar and put it in a closed furnace, and drive out the gas, and so get rid of the ordinary illuminating-gas, the heavier gas, which is condensed as it passes through the tubes. The constitution of the coal-tar depends, first, upon the constitution of the coal; in the second place, upon the temperature at which the gas is driven out. The object is to get out the lighter parts of the coal-tar. Coal-tar is a very composite substance. The physical qualities of coal-tar vary very much, according to the temperature at which the gases were originally driven out, and, again, according to the temperature at which evaporation is carried on. It requires months of experiments, as I have already said, before one can determine what is needed. This coal-tar, when it is thus taken and evaporated, and brought down to a requisite consistency, which is a matter of experience, is taken and put into a box prepared for the purpose, and put in an oven which is closed. The tops of these vessels are covered with sand, and one thing and another, in the ordinary way, to prevent the presence of air; then the heat is

turned on; and gradually this stuff begins to boil. You cannot take up this evaporated mass and tell what will be its quality, until it has been evaporated. Then, formulating these three things, you can tell what temperature you want to use under this tar in a boiling state. Then I have devised a process of quite suddenly increasing the heat. This is done by means of a double oven, one part being heated moderately, and the other to quite a high degree of heat. When the tar no longer boils, it is suddenly carbonized. You then have to make your plate. I take this substance and cut it. It is true, it is carbon, and it is pretty hard, and there is difficulty in cutting it; so I have devised another method to free myself of the trouble of cutting the plates, and that is, to prepare the plates of a proper size. This is a process which I developed last winter. The method of manufacturing which I have described is susceptible of considerable uniformity. The character of these plates is porous, and they are of considerable value in a secondary battery.

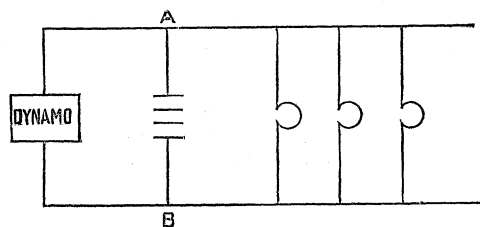
Planté, of course, is the great originator of secondary batteries of modern times. You can take a Planté cell, and you can get forty per cent from it if it is properly charged. A fair battery will return proper work up to ninety per cent. It does not always do it. It has to be carefully charged, and it will return, under careful circumstances, ninety per cent.

Mr. FRANK J. SPRAGUE. — Ninety per cent of the form: is that what the speaker has told us? I should like to know what part of the circuit he uses. Does it include the battery?

Mr. KOYLE. — Including the battery, of course.

Professor FORBES. — I merely wish to draw attention to this very point. The difficulty about manufacturing those plates is to get them of a proper length. We should know exactly what we are dealing with. The action is exactly, of course, as it has been described.

I want to show to you how the equations come out, because they are very interesting in showing the regularity of the secondary battery upon the brightness of the lamps.



Let E_1 be the electromotive force of the dynamo, R_1 the resistance of it; E_2 the electromotive force of the secondary battery, R_2 its resistance; R_3 we will call the resistance of the electric lamps in the circuit. That will represent the resistance of the lights which come into play. This is given by a well-known law. Perhaps that is the simplest way to take it. Let us call the potential at the point A, e_1 , and that at the point B, e_2 . These are the two points where all the

three circuits unite, and e_1 and e_2 are the potentials at those points. Suppose that e_1 is greater than e_2 , then the current circulating in each of these three circuits can be put down immediately in terms of the quantities which are there shown, involving the two unknown quantities e_1 , e_2 ; the current which is circulating in the partial circuit of the dynamo always taking the direction of the current as positive when the current flows from e_1 to e_2 . The current in this part of the circuit will be $\frac{e_1 - e_2 - E_1}{R_1} = C_1$,

R_1 being the resistance of this circuit. C_2 is obtained from C_1 by replacing E_1 and R_1 by E_2 and R_2 respectively, and similarly for C_3 (R_3 being the resistance of the lamp-circuit). Finally, we have the equation that the sum of all these equals zero; that is to say, the total current which passes through any one point is equal to zero, which is a well-known law. We have, therefore, the sum of these currents ($C_1 + C_2 + C_3$) equal to zero.

Now, this ($e_1 - e_2$) is in reality the unknown quantity. It is a difference of potentials. It is the single unknown quantity of this equation. The result which we arrive at on working out this equation is, that $e_1 - e_2 = \frac{E_1 R_2 + E_2 R_1}{R_1 + R_2}$. This is a very useful formula, and probably is well known, and has important applications here. The application especially is to finding the current which flows through the lamp-circuit. The current C_3 , which is the current flowing through the lamps, is equal to $\frac{E_1 R_2 + E_2 R_1}{R_3(R_1 + R_2)}$;

that is, the final value of the current which is passing through the resistance of the lamp-circuit.

Now, here is a remarkable thing, — that, however much the irregularity of the dynamo-machine may be, the current is found to be very steady in the lamp-circuit when we use the secondary battery in that position. The reason of that is, the secondary batteries which we are in the habit of using have an extremely small resistance; and, whenever we use secondary batteries, you will understand that we use an infinitesimally small resistance, compared with the resistance of the battery or the resistance of any such lamp-circuit.

If, then, we take the resistance of the secondary battery to be zero, our equation simply gives us, $C_3 = \frac{E_2}{R_3}$,

with the other quantities cut out; that is to say, the current is exactly the same as if this dynamo was not working at all, and as if we had a battery of infinitesimal resistance, with this electromotive force of E_2 working through these series of lamp-circuits.

As I am here, I may as well make a few remarks upon another point in connection with these secondary batteries. Of course, the great difficulty we have had in the past times was, in the first place, that we used thin plates, and they buckled. This has been the most serious objection that we have had. And it is very satisfactory to hear from Mr. Preece that he found that he practically gets over the buckling of the Faure cell and Volckmar battery. The

force generated in these plates, tending to buckling, is something enormous.

I do not think any ordinary separator of material which he speaks of would be sufficient to prevent the buckling of a Volckmar accumulator; for the force generated in these plates, tending to buckle them, is something enormous, and no ordinary separator would be able to resist this. If such resistance were offered to them, I think they would break. The reason why he has succeeded in getting rid of this buckling is because he has reverted to what is evidently the most scientific, and which will ultimately be found to be the most probable, form, that of the old voltaic battery. The objection to the Faure cell was the want of perfect symmetry in the plates, tending to make them buckle, and want of homogeneity in the surface, which tended to introduce local action. And that is the one great defect which has been pointed out some time ago, which I think most people recognize now in all these Faure cells and Volckmar batteries.

³ I wish Mr. Preece would give us some further information about the length of life of these cells; because although he has them in his charming house, and although they last only four months, does not everybody who will call want to obtain some more definite information? and I think there must be some gentleman here who can give us more information about the life of these batteries. As a matter of fact, the larger form of Planté battery, the improved type of batteries, which I believe were tried generally with nitric acid in order to render them more powerful, have been generally used with lead, but only within the last year or two, and therefore their absolute life is not determined; but I believe it is perfectly conceded that these voltaic batteries as they have been prepared with lead, and as Mr. Preece has them in his own establishment in his own private house, are the longest-lived batteries,—far longer lived than any artificial batteries such as the Faure cell and the Volckmar.

MR. F. C. VAN DYCK. — I do hope the people will not be discouraged in working in the line of other batteries besides the old one. I think there are cases in which the battery is sure to have a long life, and sure to keep itself charged without loss for a great length of time.

There are many uses for such a battery as that; and my own work satisfies me that the zinc alkaline and copper battery will fill a considerable want; for instance, running the magnetic apparatus in observatories, and work of that kind. I have made a small battery of that kind, using a spiral of copper above, and amalgamated zinc below, so as to avoid the falling-down of the deposit of zinc upon the copper, thereby amalgamating the copper, and rendering it far less efficient. Although the electromotive force is only ninety-seven and ninety-eight hundredths of a volt, still we found by actual trial that charging the battery and letting it stand, then letting it run down and taking the readings at the time, and then letting it stand for two or three months—we found there was not enough difference in the readings to

show the slightest loss; the battery had retained its charge, so far as we could find out by the means at our command, perfectly for two months; which I think is a good showing. Long ago I found, in using a battery, that I had to be very particular about the electromotive force to be used in charging it. I have not finished my experiments so as to state precisely the electromotive force, but it is somewhere between a Daniell and a Grove. If you charge a cell of that kind with a Grove cell, you will find that the copper strip will be altogether dissolved by alkaline solution, forming the cuprate of soda or potash, according to the alkaline used. Then that breaks up in the form of oxide of copper. But, if you are particular about not running beyond the Daniell, there is no such difficulty experienced.

I want to say a word on the carbon question. I am interested to know whether anybody has succeeded in producing oxygen on carbon to any considerable extent without disintegrating it. I have had great difficulty, in my experiments with the secondary battery, in getting oxygen on my carbon plate, and depositing the oxide of lead. I have found, if I succeed in bringing forward the oxygen in the slightest degree, it leads to the disintegration of the carbon by means of the oxygen. Hydrogen does not do it at all. Now, if we place a carbon disk with a platinum disk in an ordinary Volckmar battery, we will soon discover how long that carbon will last. The oxygen seems to have the power of forming an accumulation in the pores, and crowding out the carbon, more than the hydrogen does. I dislike to have any thing to do with the lead-battery mentioned. I think that this matter of the copper-battery is worthy of passing mention, because I can see those cases arise in which we should like to have a battery that we could charge, and know we could depend upon, and that would not lose its charge even after two months. I am positive that anybody who tried it would be pleased with the properties of the copper-battery made in the usual way, with amalgamated zinc alkali, either soda or potash. Preference should be given to soda on account of its being so much cheaper. The method should be to use the Planté battery with chlorate of sodium in it. I think the tendency of the copper to dissolve, and to break up into cuprate forms, is not any greater when the soda is used.

MR. KOYLE. — I just wish to inquire of Professor Van Dyck how his carbon is made: it makes considerable difference. I should like to know the manner in which the carbon plates are formed. If they are formed by the ordinary method for use in the primary batteries, the carbon being made out of coal-tar carbonized, they are very brittle, and disintegrate in the battery; but if formed in the manner I speak of, from coal-tar, pure and simple, so that they seem to form a very much more homogeneous mass, there is not only a striking, but a permanent difference of the plates.

PROFESSOR VAN DYCK. — I will state that I use the commercial carbon simply.

MR. KOYLE. — Another word. I think that there is a great deal of difference between the secondary

battery composed of carbon plates and one composed of lead plates. I do not see myself the reason for an expression of the opinion that the Planté battery is decidedly more scientific than any other batteries composed of a plate of polarizing substance distinct from a plate of lead. It is easy to construct a Faure battery that will last forever, if you only make the plates large enough. Lead has some weight, as is well known; and, when you are going to make a Planté battery of sufficient size to last any length of time, you will have to make a secondary battery of enormous weight. For any other purpose than putting it down in the cellar to stay, the weight is a great factor. I think that a lead plate a foot square, the ordinary size, will weigh something in the vicinity of six pounds, — I do not guarantee that exactly, but I believe it is in the neighborhood of six pounds, — and a square of carbon plate of the same size weighs about three-fourths of a pound. Now, a battery made of eleven plates of lead of this size gives you something like sixty or seventy pounds. If you want to use electromotive force, as in some cases you do, of a hundred volts, you have a great weight: it would be fifty of those cells, of sixty pounds each, which would give you about three thousand pounds; and that is altogether apart from the weight of the electrolyte and the box. Carbon plates will make a difference of about twenty per cent, and the carbon plate has the advantage of always remaining intact. It will not disintegrate, and will remain permanent as long as the box will, or any thing else about it.

MR. ELIHU THOMPSON. — I should like to have a method which will tell whether the plates will disintegrate or not. Consider the plates that received the deposition of peroxide of lead. Suppose we were to select one particle of oxide which has been formed from the surface of the lead: what will be the electro-chemical conditions of that particle? Evidently the peroxide itself is in an electro-negative condition, far more so than the lead plate: consequently it will set up an open circuit, and distribute its oxygen more or less to the surrounding lead, and thereby eat into the lead deeper and deeper, destroying the life of the battery in time. It would seem, from this consideration alone, that the lead-battery is certainly limited in its life.

Let us take the case of the carbon plates. Has it yet been proved that carbon is stable in the presence of peroxide of lead, in contact? Perhaps this may disintegrate the carbon, and oxidize it into oxide of carbon, and at the same time reduce the peroxide of lead to the teroxide of lead (PbO_3).

We have here two different substances, one highly electro-negative; and the carbon, perhaps, may be inert, and then it may be different in electromotive force ever so slightly, and we will have a local circuit which will take oxygen from the peroxide of lead, and oxidize the carbon. If we wish to make a test of that, let us take a massive piece of peroxide of lead and a specimen of the carbon which is to be tested, and place them in an electrolytic liquid (for instance, dilute sulphuric acid), and see whether we have a difference of electromotive force between

those two substances, putting in the circuit some means of testing a feeble current. If there is no such difference, the stability of carbon in contact with the peroxide of lead would be established. It may be, also, that the states in which carbon is known to exist may control the matter to some degree. It is well known, that if we take a stick of carbon, and put a current through it of a sufficient degree to almost vaporize the carbon, it undergoes a certain change of condition into the graphitic variety, and, in fact, it takes a plastic form, as I have often observed. You can take this carbon and use it, or any portion of it, to write with as you would write with a lead-pencil: whether it would be suitable in that form is another question.

PROFESSOR JAMES DEWAR. — There is one point I have noticed that is of practical use in reference to secondary batteries, and that is as to the inequalities in the various styles of batteries. I have had an experience of six months with one form of storage-battery, — the commercial battery as it is now in use on a lamp-circuit. As it is arranged, it is used for lighting forty incandescent lamps. We have only ten or twelve of these lamps in use at any one time: consequently the demand on the battery is not very great; and hence, in charging the battery, we find with a twenty-ampère current, which is the current used in charging them, that instead of having to charge the battery all day, as I believe would have to be done in case the whole forty lamps were to be used, we charge for two hours in the morning, or until the cells give off gas from the plates, when we consider the charging as completed. I believe, after three weeks' use of this battery, that some four or five of the cells that were to serve in the first place became feebler than usual; that is, the lamps were not up to their usual standard of power. The next morning it was found that certain of these cells, four or five of the twenty-one that formed the battery, were much lower in their electromotive force than the other cells of the battery; that is to say, if the mean of the electromotive force was 1.85 volts, they were down to less than one volt. It was thought, in the first place, these cells might, perhaps, become exhausted by short circuits; but, from very close examination, there were revealed no short circuits in the cells; and, furthermore, when the charging current was put on again, those cells were the first of the whole battery to give evidence of being charged. In other words, it would seem as if the storage capacity, if I may use that expression with regard to those cells — the storage capacity of these particular cells had been diminished in some simple way.

Now, we restored those cells by a process, — the process which we were instructed to use in the first place. A battery was first set up; that is to say, a prolonged charging of the whole body, — a charging of some twenty hours with a current of twenty amperes, and then a discharging of the whole battery on a fixed resistance, which was about equal to the ultimate resistance (say, eight-tenths of an ohm); then a charge of twenty hours with a twenty-ampère cur-

rent, and again a discharging the battery for three or four hours on this fixed resistance; then a re-charging for twenty hours; and at the end of that time the battery was in good condition again, and those particular cells which we had found had given out before did not give out again for several days; but in every case, I think, where that process has been repeated three or four times, it finally took about three weeks for these batteries to give out. I don't remember exactly now whether the same cells are involved, but I think that out of the five or six cells, four were involved every time. We occasionally find another cell going in the same way, and I think it is a question of time when all the cells will probably behave in the same way: otherwise the cells which are good, and which have not given any trouble, are good cells; and there is no doubt about the resistance of the whole battery. The electromotive force is very constant, with the exception which I mentioned, and the light is very satisfactory. But I am not at all sure that Mr. Preece has found a solution of the light in question by means of the secondary battery. It is to be hoped that he has. My experience rather indicates that we have not got a storage-battery, or a secondary battery, which will be a practical instrument.

MR. PREECE.—I should like to reply to some of the observations that have been made; and, in the first place, I should like to corroborate what has been said about the percentage that the storage-battery would utilize in shape of light.

Very recently Professor Dewar was kind enough to make a calculation for me that brought out a very interesting result, and the result was this. I wanted to know the relative proportion of energy expended in different modes of artificial illumination. I wanted to know how much was expended in the sperm-candle to give one-candle light, and how much in a gas-flame, and how much in electric lamps. The result of Professor Dewar's calculation was to show that in a sperm-candle for one-candle light we expend ninety-seven watts. The experiment with gas showed, that, for every candle given out, sixty-two watts were consumed. Now, I have been experimenting with various incandescent lamps; and one lamp gave a result showing that it was possible to obtain one candle for two watts and half. Still with arc-illumination—use of arc-lights, such as are used in the street—we get a candle for each watt expended; and in the arc-lamp, when bereft of the hideous structure put around them to destroy or reduce their light, and we get all the light emitted, we shall probably get a candle for each half-watt. If we can get one candle for the expenditure of one watt, and we do expend ninety-seven watts in the process, it is clear that ninety-six watts must have been wasted, as it were, in that effect.

There is another point that arises out of this, to which I want particularly to call your attention; although I am afraid I am going to get into a subject in which, if introduced, many of you may set me down as very heterodox. My doctrine is simply this,—that I believe the days of the arc-light are numbered, and that all the lights in the future will be furnished

by the incandescent lights. My reason for saying that is simply this,—that, up to the present time, in the incandescent lamp such as we have, the light is produced by the expenditure of from four to five watts per candle. Improvements, and very rapid improvements, are being made in the form of the carbon filaments. We are now using lamps in England that give us light with an expenditure of only two watts and a half per candle. If improvement goes on at this rate, I am quite certain that before another decade we shall have incandescent light that will give us one candle for each watt. When that is the case, then the incandescent lamp will be used in place of the arc. The arc requires constant personal supervision. It requires mechanism to keep it in order. It only lasts a short time, whereas the incandescent requires no attention whatever after it is put up; and its life is very considerable indeed. In London, lamps can now be obtained whose life is guaranteed to be a thousand hours. I believe on this side of the water they are guaranteed to a certain extent, I do not know what; but if we can get incandescent lamps giving a light at the expenditure of a watt per candle, and whose life will be over a hundred hours, then it will be a case of good-by to the arc-lamps.

There was one other point that I did not mention in regard to my battery; and that was, I am using only seventeen cells. I am using only thirty volt lamps, and I use them for security, first, because it gives me very few cells to keep in order; and, secondly, because the electromotive force is so low that there is not the slightest fear of shock, and consequently there is not the least fear of fire due to a short circuit or imperfect action in any of the insulation. In isolated houses, the lower you can reduce the electromotive force, the safer it will be.

Now, with regard to carbon: I am sorry to say that I differ altogether from the view that has been expressed, that carbon is likely to replace lead for secondary batteries. Carbons do disintegrate with us in London to a very large extent in the present form of battery. I had the bi-chromate battery: the battery we principally use is a bi-chromate battery. In that battery the carbons do not last more than twelve months. They do disintegrate: they tumble to pieces, they become quite soft and spongy; and it is quite impossible to use any form of carbon,—moulded carbon; and we are obliged to use cut carbons, as the moulded carbons would not last more than two or three months. While the cut carbon does not last more than twelve months, the lead in the secondary batteries would last more than twelve months. I said that I did not agree with the remark that was made, the statement that it lasted only four months: I think it will last more than twelve months. I know of houses where they have been in use for more than twelve months, and I am quite satisfied that the lead used in the one I have described will have a durability of more than twelve months. I shall not be the least surprised if it lasts for two or three years.

Now, I look upon the employment of the secondary battery for starting the gas-engine as simply

barbarous. I say this because I believe that the whole—I should not say the whole, but the greater portion—of the causes that have brought the secondary batteries into such ill repute are due to the various barbarous practices that have been adopted by those who have been using secondary batteries. And these practices have been indulged in without the person knowing the injury they were doing to the battery. The practice has been to dash in a piece of metal in order to see the sparks, and then to say what splendid order the battery was in. A more iniquitous or sinful practice could not be adopted. It is just like a doctor cutting off a man's forehead to examine his tongue. Such a practice as that has given the poor battery a straight blow between the eyes, which it will struggle with for days. Any practice that constantly calls into action the force of the battery on such a short circuit is simply barbarous, and tends to destroy the battery more than any thing else. I test every cell of my battery every morning with a galvanometer of a hundred ohms resistance. The galvanometer is simply run through on the cells: it takes but a short time, and you can see exactly the condition of the cells without in any way interfering with the condition of your battery.

There is one point about which I should like to have Professor Dewar tell us a little something. There is one defect, and it is a very peculiar defect, in all of these forms of secondary batteries. It follows, after a short time, from covering the plate with minium, and it is the formation of trees on the plates. That formation is observable in that form and type of battery, and it is not being observed with the *Planté*. *Planté* himself has never suffered from the formation of trees; nor have I, in the batteries I am using, seen the slightest sign of these trees: therefore I think that the mode of separating, to prevent buckling, in my case will be a cure. It will not prevent treeing; and therefore it will not cure that defect, which is one of the most serious defects of the *Faure* battery.

Now, I will not say any thing about primary batteries. I mentioned, when first speaking, that there had been a good deal of interest expressed about secondary batteries and their introduction; but there has been none expressed about primary batteries. Such a battery as we have to-day has been brought before the London public; and it has been shown that the products of the battery formed in its action will repay the cost of the battery, and that the products can be sold for more than they cost: therefore it has been suggested, that, if it be true, it would be a splendid thing for the government to buy up all these batteries, and to use them, and in that way to pay off the national debt. A great many experiments have been made; and the results of these experiments are not to be discarded. They are successfully used for certain purposes, but they are not just yet going to knock out of the field secondary batteries in the way that has been described before us.

The qualities to which Professor Dewar referred are due to the impurities of the lamp. I have suffered somewhat in the same way. I have cured it

precisely in the same way by putting on the power for twenty hours; and in that way the impurities, or whatever they may be, have been jostled out. The result has been, that this has had to be done about every two months, whenever there was a repetition of the difficulty. What I am going to do is this: I am going to have my battery in such order that I shall devote one day to the charging of the battery; and my gas-engine will be going all day long, and that will charge up my battery; and I shall have on that day sufficient storage-power to enable me to keep my house lighted for the rest of the week.

Professor JAMES DEWAR. — My views with respect to the chemistry of secondary batteries may be shortly expressed as follows: I feel that, in the future, some other body than the peroxide of lead will be discovered, which will more efficiently represent the amount of energy absorbed. I take it to be, that, after all, it is the question of the relative efficiency of such batteries which is the real question under discussion. The electrolytic action is, after all, the question we are to discuss. Now, it seems to me, apart altogether from the difficulties of local action, the question is whether any other chemical bodies likely to be formed during electrolysis will be as efficient as the peroxide of lead in the construction of secondary batteries. Let us take the case, then, of the type of these reactions. In ordinary cases of chemical action, we have often two actions taking place. Take, as an illustration, the formation of chlorate of potash. As a matter of fact, in this case an exothermic and an endothermic action take place side by side. The total action takes place, like the majority of chemical actions, with a considerable evolution of heat; but the evolution of heat is, in this case, due to the formation of the chloride of potassium, and not to the formation of the chlorate. A chlorate would be endothermic, would be minus, or there would be a reduction of the temperature during the production of such bodies: therefore the energy for the formation of the chlorate is really in some miraculous way extracted out of the energy produced by the direct formation of the chloride of potassium. The production of peroxide of hydrogen during electrolysis resembles that of the chlorate of potassium. It is endothermic, formed with a considerable absorption of energy, and consequently can decompose into water and oxygen again with an evolution of heat. If we could construct a battery in which all the oxygen is fixed in this way in an unstable body, there is no question but we could produce by this means a much higher electromotive force in secondary batteries. If we take the case of electrolysis of salts, we know very well, that, in a great majority of salts used for ordinary purposes, the electromotive force is practically constant, while that accords with the well-known fact, that the thermal value of the formation of the majority of soluble salts is nearly independent of both the acid and the base; that is to say, it is nearly constant, giving something like fifteen thousand gramme units per equivalent. For a direct battery, therefore, comprised of soluble oxide and soluble acid, the electromotive force is practically

constant. To get a higher electromotive force, we must produce bodies like permanganic, chloric, or chromic acids, or peroxide of lead. It would seem, during the electrolysis of water, peroxide of hydrogen is produced in very small quantity. The peroxide-of-hydrogen reactions, which are so easily recognized in an acid fluid which undergoes electrolysis, do not belong to it, but come from a new acid, called persulphuric acid. It seems that persulphuric acid is the first product of the process, and that this persulphuric acid is undoubtedly the agent which is generally formed, and is often characterized as the peroxide of hydrogen. That being the case, therefore, we have to consider cases where we can have a reverse or converse reaction. The action of iodic acid on hydriodic acid is a well-known case. There is undoubtedly a considerable dissipation of energy in the genesis of iodic acid, because it is not endothermic, like chloric acid. The moment the hydriodic acid is mixed with the iodic acid, free iodine and water result. The original constituents appear again without the complication of secondary reactions. It is reactions of that kind that we require. It is to the question of discovering reactions of that kind, without dissipation of energy, that we shall in the future look for the further advance of secondary batteries.

With reference to what has been stated regarding the economical working of the incandescent lamp, it is quite clear that such favorable results can only come from working with an incandescent filament at a very high temperature. There is no reason why the incandescent filament should not yield the same efficiency as the arc. Some years ago I made a series of experiments as to the increase of the amount of luminous intensity with the temperature. Since that time, experiments have been made by Violle and other experimenters. All the results seem to prove that as an average and fair way of representing the facts, sufficient in the mean time for practical purposes; that, above a temperature of 500°C ., the mean rate of increase of luminous intensity is as a sixth of the power of the temperature.

This law enables us to calculate the temperature of the incandescent filament. There can be no doubt of the explanation of the advantage of working at higher temperatures. The greater the temperature of the filament, the higher is the mean refrangibility of the emitted light. If blue light is considered, instead of the increase being as the sixth power of the temperature, it moves up to the seventh. In case of the violet rays, the rate of increase would move up to the eighth power of the temperature, and consequently the percentage of rays of high refrangibility is greater at higher temperatures. I still think that there are a great many points in connection with the chemical constitution of the secondary battery which require further investigation.

MR. KEITH. — I would like to ask Professor Dewar whether he has investigated, either theoretically or practically, the decomposition and re-composition of the sub-salts of lead, such as I considered some time ago, and were the subject of discussion. I think he

was not present at the time when I stated my own experiments in that line.

MR. DEWAR. — No, I regret to say that I have not.

MR. KEITH. — Taking the solutions known as Gouard's solutions, or sub-salts of lead, which are all very soluble, the peroxide is readily formed and dissolved and re-dissolved in that solution, as my own experiments have shown: so far, I have carried them into practical effect only in laboratory experiments. But I have found that all the electricity is returned as electricity; but, of course, all the energy is not returned.

MR. DEWAR. — As I understand it, the eventual action is admitted to be, that the peroxide of lead is really produced from the sulphate. All I can say is, that if you take and compress a block of sulphate of lead on a platinum plate, and if it is electrolyzed for hours, you will get a small deposit of peroxide of lead.

MR. KEITH. — I think the gentleman misunderstood my question. I want to state here, that, from solutions of sub-acetate of lead, there is by electrolytic action deposited upon one pole the peroxide of lead, and metallic lead is deposited on the other pole. The reversal of the current reverses the operation: the peroxide of lead enters the solution, and lead is dissolved. There is a difference of potentials, say, of 1.70 volts.

Prof. GEORGE F. BARKER. — I should like to point out some of the defects in storage-batteries. I made an experiment three years ago with secondary batteries, and I called attention to these experiments in the meeting of the American association at Montreal. I believe that the same thing had been found by Gladstone and Tribe; but I think that it did not excel, although they said in their paper they had seen it referred to. The cells to which I referred were not Faure cells: they were cells of the Planté type, of a peculiar construction. The lead plates were of a peculiar form, but they were entirely separated from each other. They were separated, also, by pieces of wood to prevent the buckling. Now, with regard to whether it was the impurities of the lead or not, I am not inclined to agree with Mr. Keith on that point. The lead, in the first place, was clean. I have no doubt that the claim was a good one, and that it was as absolutely pure as lead could be obtained. Beyond that, they were so placed, that, if the lead was not pure lead, any local action that would take place in the cell on account of the impurities of the material would not affect the general electromotive force of the cell: it would cause a waste of the material, but no other effect, and would not affect the resistance of the battery of this cell, nor its electromotive force.

I attribute this action to the formation of some non-conducting film over the surface of the plates, both the peroxidized plate and the metallic lead plate. There is no doubt about it, both that the metallic lead, in this particular form in which we find it in the secondary battery, is attacked by sulphuric acid, with the formation of sulphate of lead; and that there is a chemical action and reduction of the lead

in the cell, which is also attacked by the sulphuric acid, with the formation of sulphate of lead. We all know that sulphate of lead is a pretty fair insulator. I think the action pointed out by Professor Trowbridge in these cells, and the fact that there was a little sulphate formed during the operation of the cell; the fact that the cell is restored after this prolonged charging, in which, of course, there must be a great loss of energy; and the fact that they are restored to their original condition, — would induce me to believe that it is nothing due to the impurity in the lead, but rather is owing to the non-conducting film, which discolours the plate. Besides that, with regard to the short-circuiting, there is no short-circuiting in the cell. I am quite satisfied from attendant circumstances and observations, and from experiments that were made, that they saw no short-circuiting in the cells.

Before I leave this subject, I should like to say a word or two with regard to the hope that Professor Preece indulged in. I am glad that he has had such an encouraging experience with regard to storage-batteries. I am inclined to think he will be disappointed in the hope expressed, of being able to charge the cells on one day of the week, and then use them during the rest of the week. A very simple calculation would convince him, as well as the conference, that such a plan is not practicable with his present plant. Perhaps, if he increases his plant, he may be able to do it: he cannot do it with what he has on hand. If we take the figures as Professor Preece has given them, of a candle from $2\frac{1}{2}$ watts of energy, a 16-candle lamp would require 40 watts, and that gives us an efficiency of about 18.6, — 16-candle lamps per horse-power of candle-energy, which I must say, in the first place, is good lighting. 40 watts, with 30 volts between its terminals, will give us 31.3 amperes as the current to the lamp. Now, if we suppose that Mr. Preece has, in using 10 lamps, for instance, — I do not know how many there are in the plant used; we will say 10 lamps; put the number small, because 10 lamps are enough for ordinary use, — 10 lamps would require 13 amperes to maintain them. 13 amperes for 3 hours would be 39 ampere hours, we will call it, on an average night, and, for seven nights in the week, would be 273 ampere hours. Now, we divide 273 by the efficiency of the storage-batteries, — forty per cent, — and we will get as the total number of ampere hours, during which the battery must be charged, $682\frac{1}{2}$. If we divide that by the number of hours which the charging would occupy, — twelve hours during the day, — that will give us the time used in charging

the storage-battery, to produce this result, and give the current to the storage-battery of 56.9 amperes and 42 volts; in other words, 52 amperes to .8 of an ohm. The charging resistance of the battery would certainly be greater than .8 of an ohm. I call the charging resistance of the battery the difference of tension between the terminals of the battery, while the battery is being charged at the time by the current flowing in the circuit. That is what I designate by the term 'the charging resistance.' The charging resistance I am sure, from my own experiments, cannot be less than 2 ohms. Second, The current of his machine would be very materially reduced. Besides that, admitting that he could get this current of merely 57 amperes, which he requires for charging the battery, the question is, How long would his cell stand a current of that magnitude? I am inclined to think that a very few weeks' charging with a current as great as that would very soon use up his cells.

Mr. PREECE. — The assumption of the temperature he starts with there, is wrong; for I am only using ten-candle lamps, and they absorb nine-tenths of an ampere. If you go through with a calculation on paper, you are sure to make a mistake. My calculation shows that you can get two hundred ampere hours out of my battery; if so, I shall have enough to last me a whole week; and the batteries, I am quite sure, after they have been used a little time, will give me that, and a margin to spare. I am quite certain, notwithstanding those figures that I had the pleasure of going over, that I shall be able to report, that, from one day's charging, I have got my light for a week.

I will mention one fact, which I mentioned to you this morning, that my gardener cut off the top of his foot: at that time my battery was charged up Friday morning. I had a dinner-party Friday night; and whenever we have dinner-parties we make all the show we can. On Saturday I had no dinner-party, and I used my lamps as usual. On Sunday I used my lights as usual. On Monday morning my man cut off his toe. I had a dinner-party on Monday night, and I did not know of the accident at all. I got down just in time to dress for dinner. Without saying a word to anybody, I trusted to my battery; and I found my battery held on for the whole of that night. I had, without any further attention to the battery, sufficient light for two dinner-parties and four days. And I think if we did that in an emergency, with a little gentle care that could be given, I shall not be far wrong in saying that I shall have enough for a week's use.